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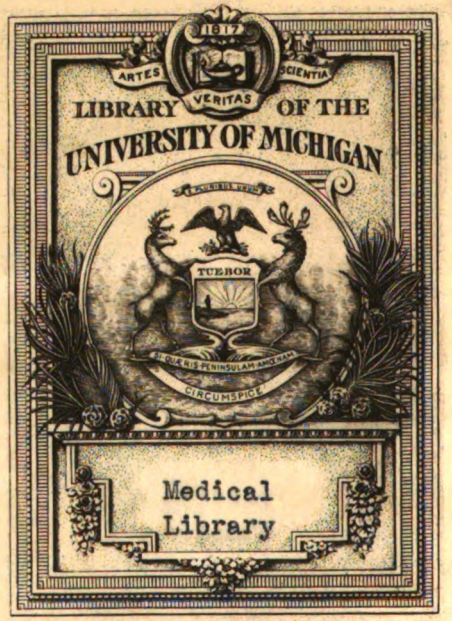

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JOSEPH RILUS EASTMAN
PRESIDENT 1915

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TWENTY-FIFTH ANNUAL MEETING

TRANSACTIONS

OF THE

WESTERN SURGICAL ASSOCIATION

Twenty-Fifth Annual Meeting

Des Moines, Iowa, December 17 and 18, 1915

MINNEAPOLIS
PRESS OF THE JOURNAL-LANCET
1916

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OFFICERS AND EXECUTIVE COUNCIL, 1916

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EX-PRESIDENTS AND MEETING PLACES

PRESIDENT	PLACE	YEAR
S. S. Todd*	Topeka	1891
Milo B. Ward*	Kansas City	1892
Milo B. Ward*	Des Moines	1893
Lewis Schooler	Omaha	1894
John E. Summers, Jr.	Kansas City	1895
Thomas J. Beattie	Topeka	1896
Joseph Eastman*	Denver	1897
David S. Fairchild	Omaha	1898
Homer C. Crowell	Des Moines	1899
O. Beverly Campbell	Minneapolis	1900
August F. Jonas	Chicago	1901
James E. Moore	St. Joseph	1902
Alex. Hugh Ferguson	Denver	1903
Charles H. Mayo	Milwaukee	1904
Harvey D. Niles*	Kansas City	1905
Malcolm L. Harris	Salt Lake City	1906
Charles W. Oviatt*	St. Louis	1907
William W. Grant	Minneapolis	1908
Arthur L. Wright*	Omaha	1909
John P. Lord	Chicago	1910
Amos W. Abbott	Kansas City	1911
Lewis L. McArthur	Cincinnati	1912
Jabez N. Jackson	St. Louis	1913
Byron B. Davis	Denver	1914
Joseph Rilus Eastman	Des Moines	1915

*Deceased.

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ARTHUR T. MANN, Secretary-Treasurer.

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Executive Session

DECEMBER 17, 1915—FIRST DAY—MORNING SESSION

The Association met in the Banquet Hall of the Chamberlain Hotel, Des Moines, Iowa, at 9:00 A. M., and was called to order by the President, Dr. Joseph Rilus Eastman, of Indianapolis, Indiana.

Dr. Oliver J. Fay, Chairman of the Committee of Arrangements, was introduced, and made the following oral report:

REPORT OF THE COMMITTEE OF ARRANGEMENTS

Mr. President and Members of the Western Surgical Association: I assure you that the profession of Des Moines is extremely glad to welcome you here at the twenty-fifth annual meeting. The profession of Iowa is so glad to welcome you that I wish to announce the entertainment we have in store for you is being directed by the members throughout Iowa, so that any good things you may have will not come altogether from the local committee of arrangements, but from the good fellows that live in Iowa. Please do not forget that you are being entertained by the members of the medical profession of Iowa, and not by the local committee in Des Moines.

Our arrangements are simple because our city is dry. We have lots of grape juice, and we welcome you in that spirit.

At twelve o'clock today the members and guests are invited to a luncheon to be given at the Des Moines Club. In this hall tonight, at 8:00 o'clock, the banquet will be held, and we will try to entertain you as best we can. The owner of this building was so glad to have you meet here that he has dedicated this beautiful hall to your service. It is the first time that any meeting has been held here. (Applause.)

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Dr. Daniel N. Eisendrath, of Chicago, Illinois, read a paper entitled "Ascending Renal Infection," which was discussed by Drs. Beck and McArthur, and the discussion was closed by the essayist.

Dr. Emil G. Beck, of Chicago, Illinois, read a paper entitled "How to Locate Foreign Bodies in the Chest and Methods for Their Extraction," which was discussed by Drs. Littig, Schwyzer, Mayo, McArthur, Fuller, Porter, and the discussion was closed by the essayist.

Dr. Frank J. Corbett, of Minneapolis, Minnesota, followed with a paper entitled "The Use of Epinephrin and Transfusion in the Treatment of Shock," which was discussed by Drs. Hertzler, Harris, Seelig, Byford, Bloodgood, Grant, and Binnie, and the discussion was closed by the essayist.

Dr. L. L. McArthur, of Chicago, read a paper on "Thrombo-Angeitis Obliterans (Intermittent Claudication)," which was discussed by Dr. Stokes, and the discussion was closed by the essayist.

Dr. Joseph Ransohoff, of Cincinnati, Ohio, read a paper entitled "Dislocation of the Knee," which was discussed by Drs. Hare and Halstead, and the discussion closed by the essayist.

Dr. Edward Starr Judd, of Rochester, Minnesota, read a paper on "Foreign Bodies in the Urinary Bladder," which was discussed by Drs. Fay, Hill, Lemon, and Brockman.

Dr. Theodore F. Riggs, of Pierre, South Dakota, read a paper on "End-to-End Anastomosis of the Bile Ducts," which was discussed by Drs. Stokes, McArthur, Mann, Mayo, and Eisendrath, and the discussion was closed by the essayist.

On motion the Association adjourned until 2:00 P. M.

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FIRST DAY—AFTERNOON SESSION

The Association re-assembled at 2:00 P. M., and was called to order by the President.

First Vice-President, Dr. Van Buren Knott, took the chair, and President Eastman delivered his address. His subject was "The Old Art and the New Science of Surgery."

Dr. James E. Moore, of Minneapolis, Minnesota, moved that the Association extend a rising vote of thanks to Dr. Eastman for his interesting and scholarly address. The motion was seconded and carried.

Dr. Arthur T. Mann, of Minneapolis, Minnesota, read a paper entitled "Nails and Screws in Joint Surfaces," which was discussed by Drs. Corbett, Andrews, Nicholson, Lemon, Eisendrath, Beasley, and Fuller, and the discussion was closed by the essayist.

Dr. James E. Moore, of Minneapolis, Minnesota, read a paper entitled "Osteomyelitis Involving the Hip-Joint," which was discussed by Drs. Schwyzer and Lewis, and the discussion was closed by the essayist.

Dr. Major G. Seelig and Dr. Don R. Joseph, of St. Louis, Missouri, contributed a joint paper entitled "Experimental Data Opposed to the So-called Exhaustion Hypothesis of Shock." The paper was discussed by Drs. Hertzler, Andrews, Nifong, Harris, Wetherill, and Byford, and the discussion was closed by Dr. Seelig.

Dr. Charles H. Mayo, of Rochester, Minnesota, read a paper entitled "Repair of Small Vesico-Vaginal Fistula," which was discussed by Drs. Byford, Moore, and Percy, and the discussion was closed by the author of the paper.

Dr. Joseph C. Bloodgood, of Baltimore, Maryland, contributed a paper on "First Aid Conference," which

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was discussed by Drs. Mayo, Hopkins, McCrae, Plummer, and Kreider.

On motion the Association adjourned until 9:00 A. M. Saturday.

DECEMBER 18—SECOND DAY—MORNING SESSION

The Association met at 9:00 A. M., and was called to order by the President.

Dr. C. E. Tennant, of Denver, Colorado, read a paper entitled "A Paradox in Cancer: A Case with Slides and Photographs."

Dr. A. E. Halstead, of Chicago, Illinois, followed with a paper on "Cancer of the Rectum."

Dr. Miles F. Porter, of Fort Wayne, Indiana, followed with a paper entitled "Further Studies of Malignant Disease of the Ovaries."

The above three papers were discussed together by Drs. Ricketts, Freeman, Mayo, Eisendrath, Davis, Pearse, Andrews, McCrae, Reynolds, and Reed, and the discussion was closed by Dr. Porter.

Dr. Dean Lewis, of Chicago, Illinois, read a paper on "Bleeding Nipple."

Dr. W. W. Grant, of Denver, Colorado, followed with a paper on "Cancer of the Lip."

Dr. Henry T. Byford, of Chicago, Illinois, read a paper on "Etiology and Prophylaxis of Cancer."

Dr. A. C. Stokes, of Omaha, Nebraska, read a paper entitled "The Recurrence of Carcinoma in the Neck in a Skin-Transplant."

Dr. Joseph C. Bloodgood, of Baltimore, Maryland, spoke of "Recent Developments in the Cancer Problem."

The above five papers were discussed together by

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Drs. Davis, Beck, Porter, Freeman, Jackson, Lewis, after which the discussion was closed by Dr. Grant.

The Secretary-Treasurer, Dr. Arthur T. Mann, presented the report of the Executive Council as follows:

REPORT OF EXECUTIVE COUNCIL

There were present at the meetings of the Executive Council on December 18, Drs. Davis, Jackson, McArthur, Grant, Harris, and Mann.

The following are recommended for membership:

Otis F. Lamson, Seattle, Wash.
R. H. Jackson, Madison, Wis.
A. L. Blesh, Oklahoma, Okla.
C. B. Lyman, Denver, Colo.
O. M. Shere, Denver, Colo.
F. G. Dyas, Chicago, Ill.
O. S. Fowler, Denver, Colo.
W. S. Sutton, Kansas City, Mo.
Wm. T. Coughlin, St. Louis, Mo.

The following are recommended as nominees for the Board of Governors of the American College of Surgeons:

M. L. Harris,
J. N. Jackson,
H. G. Wetherill.

The following are recommended as officers for the coming year:

President, Lawrence W. Littig, Davenport, Iowa.
First Vice-President, Arnold Schwyzer, St. Paul, Minn.
Second Vice-President, Henry H. Sherk, Pasadena, Cal.
Secretary-Treasurer, Arthur T. Mann, Minneapolis, Minn.

For the Executive Council:
Van Buren Knott, Sioux City, Iowa.
Lewis L. McArthur, Chicago, Ill.

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The Treasurer's Report was approved, and is as follows:

TREASURER'S REPORT

RECEIPTS

Balance on hand, December 19, 1914	$294.53	
Fees and dues to December	1,420.00	
Guest tickets to banquet	100.00	
Total		$1,814.53

DISBURSEMENTS

Paid for projection lantern two days at Denver	$15.00	
Paid for reporting the Denver meeting	155.00	
Paid for printing	38.00	
Paid for 250 volumes transactions	715.96	
Honorarium to Secretary	100.00	
Postage, express, etc.	18.13	
Buttons	25.19	
Banquet at Des Moines	545.00	
Total		$1,612.28
Balance on hand		202.25

As a place of meeting for next year, Indianapolis, Ind., is recommended, with Joseph Rilus Eastman as Chairman of Committee of Arrangements, and the date, Friday and Saturday, December 15 and 16, 1916.

The following resolution is offered for action by the Association:

Resolved, That the Western Surgical Association hereby endorses the effort being made to standardize medical examinations and render uniform the requirements for the licensing of physicians in the United States.

ARTHUR T. MANN, Secretary.

THE PRESIDENT: What will you do with this report?

DR. A. W. ABBOTT: I move its adoption as read, including the recommendation for a National Board of Examiners. The motion was seconded and carried.

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THE PRESIDENT: Are there any resolutions to be brought before the Association at this time?

DR. SAMUEL C. PLUMMER: I would like to offer the following resolution:

RESOLVED, That the President be instructed to appoint a committee of five to be known as "First Aid Committee of the Western Surgical Association," to make a survey of the first-aid question, and to act in an advisory relation to the Board of Standardization of the American First Aid Conference.

A motion that the resolution be adopted was seconded and carried.

THE PRESIDENT: I will appoint Dr. John E. Summers and Dr. Roland Hill as a committee to escort the newly-elected President to the Chair.

Dr. Littig, in accepting the Presidency, said:

Members of the Western Surgical Association: You heard the President's injunction to be brief. There are moments in a man's life when normal cerebration is absolutely impossible.

I cannot help but feel unworthy of the great honor that you have conferred upon me. I only hope and pray I may be able to transmit to my successor the beautiful escutcheon of this organization, which has been emblazoned and brightened by a galaxy of illustrious predecessors and by your attainments in practical and scientific fields. I know that the scientific work of this organization will not be imperiled in any way by the efforts of your President, more or less feeble, and relying upon the staunch support of that wheelhorse of this organization, Arthur T. Mann, I have the temerity to accept this honor.

I thank you from the bottom of my heart. (Applause.)

Dr. Horace G. Wetherill, of Denver, Colorado, presented the following report as Chairman of the Committee on Hospital Efficiency and Hospital Standardization, appointed last year:

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To the Western Surgical Association.

Gentlemen: Your Committee on Higher Hospital Efficiency and Standardization desires to report that it finds a sentiment favorable to the establishment of better hospital conditions prevalent among the members of the medical profession, but that the necessity for organization in this direction appears to be appreciated particularly by the surgeons and certain surgical associations. We have ascertained that definite movements for the attainment of these ends have been inaugurated and that in some localities, particularly in the East in the vicinity of Philadelphia, New York, Boston, and Baltimore, some progress has been made. At a recent meeting of the Clinical Congress of Surgeons in Boston a report was made by Dr. Earnest Codman, who as the Chairman of a Committee of the Congress offered for consideration and recommendation a plan for hospital standardization. His report considers the matter carefully and in detail, and recommends a specific classification of cases and end-results, which, he believes, would promote greater efficiency in hospitals through the establishment of a uniform case-recording and "follow-up" system. Dr. Codman's report was endorsed by the other members of the Committee, and was discussed by Dr. Edward Martin, of Philadelphia, and by the Chairman of your Committee, as the representative of the Western Surgical Association.

In Pennsylvania the Bureau of Medical Education and Licensure, under the direction of Dr. J. M. Baldy, has done much to improve hospital conditions in many of the hospitals of the state through a requirement that those institutions receiving state funds shall establish clinical and pathological laboratories, and an anesthetic department to be approved by the Bureau of Medical Education and Licensure. A full set of instructions and requirements issued by this Bureau is appended to this report.

The Philadelphia County Medical Society, through a committee headed by Dr. Edward Martin, recently made a report which was very widely circulated, containing certain specific recommendations for hospital standardization, which may be found in the transactions of the Society. They have also taken up with the Carnegie Foundation the question of doing for the hospitals of the country what has already been done for the medical schools, and have received from the Carnegie

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Foundation assurance that this matter is under consideration, and will be given attention in due course.

The Chairman of your Committee has also had some correspondence with Mr. Clyde Furst, the Secretary of the Carnegie Foundation for the Advancement of Teaching, and under date of October 13, he assures your Committee that Dr. Pritchett "is very much interested in the problem, and hopes that the Foundation may later be able to undertake it."

In a recent article published in the Journal of the American Medical Association (November 6, 1915, pp. 1615-1618) Dr. Lawrence Littig, of Davenport, Iowa, a member of our Association, makes many excellent suggestions regarding higher hospital efficiency and standardization. These suggestions are constructive in type, and refer to such essential elements of hospital organization as "the attending staff," "the training-school," "the nursing staff," "the employment of an efficiency expert," etc.

So far as your Committee is able to ascertain the best organized effort for the promotion of higher hospital efficiency and standardization is being made by and through the American College of Surgeons. The American College of Surgeons is establishing an endowment fund of half a million dollars, the interest of which is to be devoted to the following purposes:

1. To enlist the co-operation of the medical schools to confer a supplementary degree of proficiency in general surgery and in various specialties of surgery.

2. To provide, by law, in each state or province a minimum of standard requirements for the privilege to perform, independently, except in cases of emergency, operations in general surgery or in any of its specialties.

3. To issue pamphlets, educational in nature, to the press, to the general public, to hospital trustees, and to the profession of medicine upon subjects of medical procedure, abuses of practice, and the whole meaning of fitness to practice surgery.

4. To issue especially to hospital trustees and members of hospital staffs a series of pamphlets upon hospital management, dealing with such subjects as (a) proper equipment for medical diagnosis,—namely well-equipped laboratories for chemical, pathological, and *x*-ray work; (b) the proper forms for case-histories and the facilities for keeping these records; (c) the management and the curriculum of the nurses' train-

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ing-school; (d) the specialization essential in any well-organized hospital.

The College is to be congratulated upon the energetic effort it is making in this direction, and it should receive the support and endorsement of every surgeon and of every surgical society in its commendable endeavor.

That much might be accomplished in individual hospitals through better organization, and a larger measure of control of all hospital matters by a competent medical staff, seems evident. We are also convinced that great good for hospitals, patients, surgery, and surgeons could be brought about in every community if it were possible to organize a joint hospital "advisory board," with representatives from every reputable hospital in each community. Through such co-ordination and co-operation many important economies could be effected, and much better administration of the affairs of all the hospitals would ensue. It is probable that the important question, who should and who should not perform major surgical operations in such hospitals, could be considered by such a joint committee, and that the recommendations of such a committee might be made effective. We commend this suggestion, and would recommend such co-operative organization for the hospitals of the communities represented by the membership of this association.

We are compelled to conclude, however, that important hospital reforms on a large and uniform scale can never be initiated or be made effective through the efforts of individual hospitals or communities. Any really important hospital regulation and reform must be brought about through pressure from without by some such body as the Carnegie Foundation, the Pennsylvania Bureau of Medical Education and Licensure, state boards of health, or the enactment of national or state laws, which leads us to repeat that the organized effort of the American College of Surgeons presents, at the present time, the most hopeful outlook for the promotion of higher hospital efficiency and standardization. We recommend the approval and endorsement of this effort on the part of the American College of Surgeons by the Western Surgical Association, and would suggest that we pledge our Association to the support of its endeavor.

(Signed) C. H. MAYO
J. E. MOORE
H. G. WETHERILL, Chairman.

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THE PRESIDENT: What will you do with this report?

DR. DANIEL N. EISENDRATH: I move its adoption.

DR. CHARLES A. L. REED: I second the motion.

The motion was carried.

Dr. James F. Percy, of Galesburg, Illinois, presented the following report as Chairman of the Committee on a Traveling Tour to Japan:

Because of the European war, your Committee has been in some doubt as to the advisability of attempting to organize a party for a Travel-Study Tour to Japan. A large part of the Pacific Ocean is in the war zone, and just what developments might occur to make a trip within this zone hazardous, was, and is, difficult to determine. Your Committee decided that the Accident and Life Insurance Companies would probably be a better barometer as to the safety of the trip from a war standpoint than any other from which we could gather information. We found that the old-line life companies, unless there was some special contract, held their policies as covering this extra hazard, in nearly all cases, if the policy had been in force two years. With the Accident Insurance Companies, however, it is different. They will not assume any risk, direct or indirect, in the war zone, in which Japan is included, nor for three months after the war is over. This, of course, refers only to the hazards of war. The ordinary accidents for which the policy was written, are still covered.

The first concern of your Committee was to determine how many of our membership would be willing to make the trip. With this in view, the following circular letter was sent out to the members:

Galesburg, Ill., November 19, 1915.

My Dear Doctor:

As perhaps you remember, the undersigned were appointed a committee to arrange for a Travel-Study Tour to Japan next year. The main purpose of this letter is to ask you to decide, between now and our meeting at Des Moines, whether it is possible for you to take the contemplated tour.

This is very necessary, because we must ar-

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range for steamer accommodations on the Pacific, almost at once. As perhaps you know, there is only one steamship line carrying passengers on the Pacific now, and that is a Japanese company. Their accommodations are going to be in great demand for the coming season, so that it is most important now for your Committee to know just how many of our members will likely go.

We will have to leave San Francisco about April first, in order to tour Japan at the most beautiful season of the year. The expense of the trip will probably be about eight hundred dollars, and this will give us everything absolutely first-class. The ladies are to be invited to go with us.

One of the difficult things to decide is how long we shall stay. To make the tour comfortably would take at least twelve weeks. This would include Honolulu, both going and coming. If we miss that place one way, it would add about one hundred dollars to the expense. If Honolulu is left out entirely, the trip will be shortened considerably. On account of the war, more can be had for our money now than after it has ended.

As it takes between three and four weeks to get a reply from Japan, you will appreciate the anxiety of your Committee to have the number who will go as nearly determined at once as possible. We expect to have a full report for you at the Des Moines meeting. All we want to decide now is—WILL YOU GO?

Respectfully submitted,

J. F. PERCY,
LEONARD FREEMAN,
JABEZ N. JACKSON.

To date we have received thirty-two replies. Out of this number three have signified their willingness to go. One of these members will be accompanied by his wife, making the party so far consist of four members.

One of the best and most experienced tourist agencies of New York was consulted as to routes, expense, and time of year best to make the trip. We learned that we should have at least a party of fifteen, and that the expense would be

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between eight and nine hundred dollars. The agency could report to us only on what we would see if we were ordinary tourists with no special object along surgical lines. Your Committee has made arrangements, however, to cover the surgical phase of the trip. The only things necessary will be, first, to get our party organized, and then take up with the tourist company our program of clinics and places to be visited because of their medical or surgical interest, and correlate them. The second important consideration is the time required for the trip. Because of only one steamship line operating on the Pacific, and that by a Japanese company, we are limited to this company's sailings. It is possible that the British Admiralty will release the C. P. R. Line departing from Vancouver before our party would want to leave. In that event, we could go out by the Japanese line and return via Vancouver over the Canadian line. Because of the uncertainty of the Canadian line being in commission, however, we had better stay by the Oriental line, for fear of not being able to get accommodations when we are ready to return.

We find that there is no way of avoiding two visits to Yokohama, Kobe, Nagasaki, and, possibly, Shanghai, in the far East, no matter what steamer services are used, as these ports are generally made both ways by all lines, even in times of peace. Of course, if the Canadian line is running when our party is ready to return, it is possible that arrangements can be made for those who wish to return by the way of Vancouver and the Canadian Rockies. The Japanese lines do not stop at Manila. If we go there, it will have to be by local steamer from Hong Kong. This will require about a week for the trip, two days each way and two days for the visit.

In order to get to Japan in the most interesting part of the season, we find that we will have to leave San Francisco Saturday, March 25. This will insure our traveling in Japan during the best of the cherry-blossom season. Returning, we will land in San Francisco June 26. This will mean an absence of practically three months from San Francisco. Of course, this can be shortened by leaving out Manila and Honolulu. The time could also be lessened a little in Japan, and this given to a trip to the Philippines and back, via Honolulu. We could also leave San Francisco April 18; but this steamer, the Nippon Maru, is one of the older ships and not nearly

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so good as the one (Tenyo Maru) leaving March 25. The Shinyo Maru leaves April 22, and, although this date is rather late, the ship is a splendid one in every way.

The most important city in Japan from a medical standpoint will likely be Tokio, because of the Imperial University. In addition, there is a fine hospital under the auspices of the Red Cross Society, as well as the well-known private hospital conducted by Baron Takati, and St. Luke's Hospital. There is also a new government laboratory in San Ko Cho, and the private laboratory of Prof. Kitazato, which will have much of interest for our party. We will probably want to spend several days in Tokio. The Government has also started some new medical work at Osaka. Your Committee has also learned that there are a number of private hospitals that are well worth seeing in some of the cities that will be visited on our tour. Kioto will also have something of interest in a medical way. We can also arrange for clinics at Yokohama and Kohy, but these are under the direction of German physicians.

The Japanese Society of America, with headquarters at New York, will aid us in every way to make our trip both profitable and interesting. In addition to this, the Japanese Government maintains a Bureau of Travel of Japan, which will facilitate our journey, adding to its profit and pleasure.

The following additional items of information should be given here. Because of the war, it is possible that the steamer sailings may be a little irregular. Again, if we find it possible to go out by the Japanese steamers, and return over the C. P. R., one hundred dollars additional will be added to the expense of our trip. The price mentioned provides for two people in a stateroom. The rule is for three. This will greatly add to the comfort of those going on this tour. The only exception to this will be that, if we go from Hong Kong to Manila by local steamers, three will have to occupy a stateroom. In Japan, if there are fifteen or over in our party, we will have a private car. We will also have a high-grade conductor. Indeed, the price contemplated for the trip insures our party luxurious accommodations in every way. We can make the trip at least one or two hundred dollars cheaper now, because of the general stagnation in tourist travel, than we could were there none of the present hindrances to foreign travel.

The details of the trip can be finally worked out by your

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Committee after you have decided at this meeting whether there are enough of our members ready to take the trip. We would also like to have some expressions from you as to the time we should give to the trip. The Committee has decided that it would be most convenient for our members to meet in San Francisco. Another thing to be decided is whether we shall invite any one outside of the membership of our Society. A party of from fifteen to thirty-five will be about right, as far as obtaining accommodations in hotels and trains with the maximum of comfort, both socially and physically.

It might be well to mention, also, that, if we go, it will be the first party of American physicians that has ever visited the Far East and the new Possessions of the United States. It is fair to presume that we will have a most cordial welcome from the government and local authorities and the members of the medical profession in the places we propose visiting.

Respectfully submitted,

J. F. PERCY
LEONARD FREEMAN
JABEZ N. JACKSON.

THE PRESIDENT: What will you do with this report?

DR. ARTHUR T. MANN: I move that the trip be postponed for at least one year. The motion was seconded and carried.

Dr. Van Buren Knott, of Sioux City, Iowa, read a paper entitled "Excision Versus Gastro-enterostomy in the Treatment of Gastric Ulcer," which was discussed by Drs. Freeman, Harris, and McArthur, and, in closing, by the essayist.

Dr. Charles A. L. Reed, of Cincinnati, Ohio, read a paper on "Chronic Acidosis as a Factor in Determining Surgical Results," which was discussed by Drs. Andrews, Christie, Eastman, Seelig, McCrae, and Jackson, and, in closing, by the essayist.

Dr. William Hessert, of Chicago, Illinois, read a paper on "Epiploitis as a Sequel of Herniotomy."

Dr. A. W. Abbott, Minneapolis, Minnesota, read a

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paper entitled "The Early Diagnosis of Intussusception in Children."

The above two papers were discussed together by Drs. Harris, Eisendrath, Schwyzer, Fuller, and Porter, and discussion closed by the essayists.

Dr. Byron B. Davis, Omaha, Nebraska, read a paper entitled "Intestinal Stasis," which was discussed by Drs. Summers, Abbott, Benjamin, and McCrae, and the discussion was closed by the essayist.

Dr. Carl E. Black, of Jacksonville, Illinois, read a paper entitled "Some Experiments with Rubber Gloves."

Dr. Emil G. Beck moved that a vote of thanks be tendered to the local Committee of Arrangements for the hospitality extended and for the many courtesies shown to the members and guests during the meeting. The motion was second and carried.

As there was no further business, scientific or otherwise, to come before the meeting, on motion the Association adjourned to meet at Indianapolis, Indiana, December 15 and 16, 1916.

ARTHUR T. MANN, M. D.
Secretary.

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President's Address

THE OLD ART AND THE NEW SCIENCE OF SURGERY

JOSEPH RILUS EASTMAN, M.D., F.A.C.S.

INDIANAPOLIS, INDIANA

Art is the application of means and methods to accomplish desired ends. Science is the systematized knowledge of principles and laws. Surgical art is old. Surgical science is new.

The most primitive surgery included all the rudiments of an art. The papyrus of Ebers from the excavations of Memphis records the application of principles and rules in surgical performances. Mummies of Luxor and Karnak of the date of 5,000 B. C. and antedating the advent of Adam several centuries show evidence of the practice of surgical art, as they have gold-filled teeth.[1] The art was taught at Thrace and Babylon, at Heliopolis, and hundred-gated Thebes. Its history parallels the history of man.

Empirical asepsis was practiced by the pupils of Susruta, a Sanskrit writer of 1,000 B. C. He[2] advised his students to keep their hair and nails short and to wear white linen coats.

At the time of the destruction of Pompeii, surgical handicraft, as evidenced by hundreds of almost incredibly modern-looking instruments and contrivances in the Naples museum, was becoming complex through

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the grouping of many mechanical ideas, though it cannot be said to have reached the stage of systematic science. If we could remove the rust and verdigris from these ancient appliances in the Bourbon museum and plate them with nickel, we should have before us an exhibition which we would find to be in many respects surprisingly and interestingly modern. Many of these devices should bear, if common impressions of priority are correct, the names of living surgeons. Here are many evidences that surgeons of hazy antiquity appropriated without credit the inventions of the moderns. For example, one may view here the trivalve rectal speculum to which we are accustomed to attach the name of a beloved Nestor of American proctology, as well as modern stone and bullet forceps and trephines with rotary but-stock handles. The later works of Guillemeau depict an instrument strikingly similar to a modern bone-holding clamp for plating operations.

Throughout nearly all of historic time surgical knowledge was purely empiric. It was art not science. So it was with the Egyptians, the Babylonians, and the Lusitanians[8] who exposed their sick in public so that if passers-by had been similarly attacked they might give advice to the sufferers.

It is said that a quasi science of surgery was born in Greece: that neither in Egypt, nor in India, nor in Palestine, nor in Persia, but in Greece alone were planted the earliest seeds of scientific culture. "There one must seek the most perfect blooms of human knowledge which in every fortunate land unfolded and bore most precious fruit." (Kurt Sprengel.)

Thus Hippocrates, son of Heraclides, descendant of Podalirius and Aesculapius, is said to have raised surgery from a system of superstitious rites practiced wholly by the priests to the dignity of a learned profession.

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It is probably true that he enriched surgery with principles and truths. In every wound he recommended nothing so imperatively as elevated position of the member, as well as a cautious diet. He taught the value of heat in treating wounds and in applying splints to fractures, and advised that they be adjusted loosely without compression, thus revealing an early familiarity with the specter of Volkmann's paralysis. This was not surgical science, rather it represented an "undigested collection of experimental notions vaguely described, disfigured by tradition and often rendered inutile by superstition and ignorance."

Scientific surgery according to the modern concept, that is, "formulated knowledge of surgical principles and surgical laws, based on biologic facts," may be said to have come into existence during the last century with the birth of the school of physiologic medicine founded by Broussais, Bichat, Roser of Stuttgart, and Wunderlich, who called pathology the physiology of the sick, and the advent of cellular pathology, with the associated new development of the ancillary surgical sciences, as physiology and bacteriology; when Virchow, casting away the vagaries of Paracelsus, "arranged in better form than had hitherto been done, a view of the cellular nature of all vital processes, both physiologic and pathologic, animal and vegetable, so as distinctly to set forth what even the people had long been dimly conscious of, namely, the unity of life in all organized beings, in opposition to the one-sided humoral and neuristical (solidistic) tendencies which had been transmitted from the mythical days of antiquity and at the same time to contrast with the equally one-sided interpretations of a grossly mechanical and chemical bias,—the more delicate mechanism and chemistry of the cell."

About the same time Pasteur demonstrated that fer-

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mentation and putrefaction are caused, not by chemical forms, as Liebig had taught, but simply by the agency of lower organisms.

Thus the pure sciences of cellular pathology and bacteriology of Virchow and Pasteur established and explained causes, principles, and laws which joined with the older applied science or art of surgery with its knowledge of phenomena and facts; and, supported by all the rapidly evolving tributary sciences having to do with the origin, structure, development, and function of living things, brought forth the newer group science or compound science of surgery.

We think of Morgagni, Magendie, Bernard, Recklinghausen, Rokitansky, Lister, and Johannus Mueller, and others of their type and generation as among the founders of the modern research science of surgery. Concerning the great surgical architects and artists as well as the philosophers of this important period, it is to be said that, however much we may admire their ingenuity in invention, or their virtuososhiip in technical performances, or their fine skill in spinning theories, we cannot catalogue them among the fathers of present-day surgical science along with Virchow and Pasteur and their sympathetic contemporaries. Thus, Lawson Tait, renowned in the annals of surgical art, cannot, if we recall his polemic to Saenger declaring bacteria to be the products of disease, be grouped with Langenbeck and Billroth, surgical scientists who were "as expert with the microscope as with the knife, and equally great with both."

The modern composite science of surgery has evolved chiefly in all the laboratories of its separate component and ancillary sciences where surgical art was, and is, also, too often a stranger.

But let us not assume that surgical art is either subordinate or enslaved to science. (Acland.) The art is

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at once in advance of the science and behind it. Here the art is in advance because it observes and makes use of clinical facts of which the science is not yet aware, "believing with strong conviction that which can neither be proved nor ignored." There science is in advance, claiming system and order and an exactness to which the art cannot even pretend.

The relations of the art and the pure science of surgery are intimate and direct. Thus the art follows in the lead of the science, though occasionally it cannot wait for rationalization. They are intimate, though empiric art does not sacrifice life while waiting for the proofs and explanations of science. The relations are direct in the case of all those modern means of research and remedy which the science daily gives to the art in the form of laws and principles of which a score of years ago there was no conception. To the translumination of the body, Röntgen brought the Crookes tube, and made shadowgraphs of living organs theretofore inscrutable. To the exploration of dark recesses of the body, optical science brought instruments of precision; and in what were inaccessible places, science holds the torch while art performs its task. The framework of surgical art is made up of accurate data of observation and experiment.[4] About these empiric data of art, science has organized and crystallized principles and laws. Thus the relation of the art and the science is intimate and direct; they are as the warp and woof of established surgical knowledge.

The closest relationship exists in the realm of established and accredited fact, for, as was said, the art confronted with emergencies cannot wait until science has established her conclusions; and science, being formulated, proved knowledge and brooking no uncertainties, shrinks from the assumptions of art. How-

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ever, they unite inseparably upon the high plane of fact. Now, as formerly, there exists in some minds a doubt as to whether the art of surgery can keep abreast of the science, sustaining itself in the close relationship with the dignity and respectability which it has enjoyed in the past. It has been said that art is a finite thing and science infinite, that the art has already attained or shall soon attain finality, but that the science will grow without limit.

Most of us will wish to subscribe to the view of Sir William Stokes,[5] who said that "since we are still young upon the earth, the progress of the art of surgery as of the other arts is as certain as that we live and move and have our being."

It was believed that surgical art had reached its last degree of perfection in Napoleon's time and so recent and worthy a surgeon as Erichson[6] declared that in many respects surgical art could advance no farther. He likened the art of surgery to the plastic and graphic arts. Thus, he says, sculpture reached its final development 2,000 years ago, and no sculptor of modern times can hope to excel the triumphs of the ancient Greek. In painting, he avers, we find the same early perfection in the work of the Italian, Flemish, and Spanish schools of the 16th and 17th centuries; and as it is with these fine arts so Erichson believed it is with certain branches of human knowledge closely allied to surgery.

He conceived the advance of surgery to have been due to three factors: first, the improvement of technic; second, the increased precision of results; and, third, the development of scientific research.

As to the exactitude of performance and as to precision of results, Erichson believed there was little chance of future progress. For the further development of scientific research he held out more hope. He

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had a most gloomy outlook upon the future of surgical art, though in the future of surgical science he glimpsed a rainbow of promise. Very old surgeons might welcome the views of Erichson as a pean of satisfaction over what they and their predecessors have done, but young men will proceed unfettered by such views of finality, so often proclaimed by the oracles and so often disproved by time.

Stokes pointed out that the arts of sculpture and painting are purely imitative, whereas the art of surgery is constructive,—an essential difference. The "doctrine of finality," he says, "might equally well have been promulgated in the time of Homer, and the view held then that poetry had reached its goal, but the production of the great epic, the sun of all ancient literature, did not interfere with the appearance of the imperishable works of Shakespeare and Dante."

Surgical art is "huge in strength and wise in works," and no less illimitable than science, for the two are integral parts of a whole and are inseparable. To look upon surgical art as a thing of finality is to view it in its narrowest sense as a mere handicraft.

Sir Bland-Sutton tells us that surgeons are of two types: craftsmen and biologists. "As craftsmen, there is a limit set to their advance, but in pathology and chemotherapy, the field of progress is illimitable." Here a line of distinction is drawn between surgical handicraft and surgical art-science, which includes investigative study and the systematic application of knowledge gained in the research laboratories.

If by study of the progress of surgical art and science we seek an answer to the question of whether the art or the science is becoming increasingly the more important, we must find that more and more it is the "leaven of science which gives habits of mental accuracy and modes of thought which enlarge the

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mental vision, and, to use an expression of Epicharmus,[7] strengthen the sinews of the understanding."

Morris and Crile designate the present as the fourth era in the development of surgery, or the era of physiology. First, in the division of Morris, is the heroic era of Hippocrates; second, the anatomic era of Vesalius; third, the era of pathology of Virchow, Lister, and Pasteur; and, fourth, the era of physiology, appropriately represented by Metchnikoff and Sir Almoth Wright. The present surgical era might with equal fitness be called the era of biology, for it is through the study of the "capabilities and the susceptibilities" and all the attributes of the normal cell complex which makes up the body—the study of the laws of life itself in all relations and of the vital processes that we will attain to that "complete truth which carries with it the antidote against the bane and danger which follow in the train of half knowledge." (Helmholtz.)

Men like Crile and Carrel, who go farthest, advance through the broad science of biology, which touches the problems of life at every point. Surgery must still be heroic; it must still use the synthetic and comparative considerations and the scrutiny of prognostic science of the Hippocratic era. Gross anatomy cannot be abridged. Pathology is still a great basal factor in rational surgical practice, but in the fourth and highest era of Metchnikoff, Wright, and Carrel, it is the biologist surgeon, standing upon all the proved knowledge of the art and of the science as well, whose outlook on Nature and all her problems is the broadest because his viewpoint is the highest.

Cushing, a surgeon of the modern era, says that as a training ground for surgery, the anatomic dissecting-room fast gives way to the experimental laboratory.

Garrod, writing of biochemistry and physiology in modern research, says: "We may wander among the

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streets of a deserted city of the past, may study the fabric of its buildings—the stones, bricks, and mortar employed in their construction—and in this way may gain some knowledge of the uses for which the several buildings were designed. Just so the anatomist investigates the structure of the organs of the dead body. Once more we may tread the streets of a city of today, may watch the movements of the crowds as they pass, the gathering and the dispersal of groups of people. We may study the arrangements for the disposal of waste products, the ways in which food supplies are brought in, dealt with, and distributed. Or we may investigate the police arrangements, the sanitary devices, and the various devices resorted to by the community for its protection. The study of the human organism on such lines is the province of physiology." We are less interested just now in the pathology of the dead than in the normal physiology of the living, and in abnormal physiology, or what Moynihan calls the pathology, of the living.

Recently the most valuable contributions to surgery have come from the laboratories of biology. We need no priestess upon a tripod to tell us that surgery of the future will look more and more for strength and inspiration to the vigorous sciences of biochemistry and physiology though it must continue to rest upon its original footing of normal and morbid anatomy, nor that the way to the most complete surgical development for any individual will lie, not only through the "blood and sawdust," but also through the "glass and brass."

The debt of surgical art to the pure science is great, and the reciprocal debt of surgical science to surgical art is hardly less. Surgical science is advancing with unexampled rapidity, and no bounds can be set to its possible conquest, yet the progression of imaginative,

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speculative, investigative science will be more sure and steady with the support of sensible and pragmatic, empiric art upon its flank.

Sir Berkeley Moynihan[8] would have us recognize that every operation the surgeon performs in his daily vocation is an experiment from which something should be learned, that every clinical observation is an addition to the sum total of human knowledge not only of the disorders of the human body, but of its orderly working—not only of pathology but of physiology. In devoting attention to the individual problem he is advancing along the road that leads to more light and a clearer, wider outlook.

We do not often see a great surgical craftsman, a gifted exponent of applied surgical art, and a genius in surgical research rolled into one. In Billroth and Senn were combined the qualities of the great executant in surgical technic and those of the laboratory zealot. They are rare examples.

Surgical art and surgical research science rarely reach their highest development in one individual; and the practical and desirable alternative has been, and will be, to associate the genius of surgical research in one individual with the gift of surgical art in another in an harmonious working union. The ideal arrangement of the future will be that which includes the establishment of special laboratories of surgical research in close association with the theaters of surgical art, and which includes the close association of both with large facilities for the study of the broader aspects of disease.

Such an arrangement as this exists in many seats of medical and surgical culture in the older sections of our country; and in our western territory, where the group idea in medical and surgical practice has been most cultivated, are numerous instances of the

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close co-operation of pure surgical science with the practical art greatly to the advantage and advancement of both. As an illustration, it may not be invidious to refer to a western institution which, ignoring the hazy arguments of surgical philosophy, has added so much to the more salutary surgical art-science. Here refinement of operative technic vies for supremacy with the detailed study of immediate and ultimate results and thoroughgoing research in morbid anatomy and morbid physiology.

In a comparison of important American centers of clinical surgery, a recent writer refers, with little grace, but with much assurance, to western surgeons as still lingering in the earliest or technical stage of development. Respectfully waiving this indictment and basing our judgment on the evidence of our short past and active present, we would admonish those who cling to these older sentiments that with the unquenchable enthusiasm which impelled the early heroes of this buoyant country—"with narrow search and with inspection deep"—our seekers are keeping abreast of the most earnest and eager in the quest of the treasure trove of science.

So many critics have spoken in commendation of surgery of western America that to this one who directs a patronizing allusion to the most distinguished of western clinics, we may, we hope without immodesty, commend the words of Phidias to Pericles:

> You, Pericles, and I, do what we will,
> Are guilty, both of us, of an offense
> That envious natures never can forgive—
> The great crime of success.
> 'Tis at the tallest poppies that men strike;
> 'Tis at fruit-bearing trees that they throw stones.

From many western surgical communities of greater worth than eminence one hears less and less of

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things and men, and more and more of ideals. Each of these helps with what light it has to dispel the darkness. "It ought to be no disparagement to a star that it is not the sun."

Into the vast and attractive field of surgical scientific research all can enter and work. If we labor and achieve, we shall have the best reward in the approval of conscience; the compensation otherwise is negligible. If we labor and fail, we shall not have labored in vain, for "the tree sucks kindlier nurture from a soil enriched by its own fallen leaves, and man is made in heart and spirit from deciduous hopes and things that seem to perish."

REFERENCES

1. Gorton: History of Medicine.
2. Hellier: The Lancet, Lond., 1910, January 8.
3. Park: Epitome of Medical History.
4. Acland: Medicine in Modern Times.
5. Brit. Med. Jour., 1886, Nov. 20.
6. The Tendency of Modern Surgery. Brit. M. J., 1886, Aug. 14.
7. Osler: Equinematas and Other Essays.
8. Brit. Med. Jour., 1913, July 26.

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ROLE OF THE LYMPHATICS IN ASCENDING RENAL INFECTION

PRELIMINARY REPORT*

DANIEL N. EISENDRATH, M. D., F. A. C. S.

AND

JACOB V. KAHN, M. D.

CHICAGO, ILLINOIS

We have just completed a series of twenty-seven experiments on dogs and rabbits which show that infection travels from the bladder to the kidneys and perinephritic tissue by way of the lymphatics in the wall of the ureter and not along its mucous membrane. Our observations, so far as we can ascertain, are the first to show that the lymphatic capillaries of the peri-ureteral sheath, in laboratory animals as well as in the human being, play a most important part in ascending infection. Going a step farther, the constant finding of evidences of infection in the immediate vicinity of the rich network of peri-ureteral blood-vessels makes it seem probable that infection can travel, as Sampson and Bauereisen assert, to the kidneys from the female genitalia and other abdominal viscera which lie in close relation to the ureter.

Franke has shown how the lymphatics of the ascending colon communicate with those of the right kidney, and thus explains some, at least, of the renal infections

* From the Morris Institute of the Michael Reese Hospital and the Department of Surgery of the College of Medicine of the University of Illinois. Experiments will be published in full later.

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in children. The assertion of Bauereisen, that cystitis and renal infection following gynecologic operations take place via the lymphatics, sounds very plausible in view of our results. The older view, that infection from the lower to the upper urinary tract travels within the lumen of the ureter, that is, against the secretory current, must, we feel, be abandoned except when the ureter is completely obstructed by a calculus, stricture, or external pressure.

In nearly all previous animal experiments, conditions were established, such as ligation of the ureter, injection into the renal pelvis, or implantation into the bladder, which seldom exist clinically. Under these conditions it is not surprising that evidences of infection are found (Aschoff[1]) within the tubules of the kidney seven hours after injection into the ligated ureter. The changes, both in the ureter and in the kidney itself, are usually so advanced under these circumstances that, as Mueller has recently shown, one cannot trace in a satisfactory manner the route along which the infection first traveled.

We have attempted to imitate the conditions as found in the human being by injecting an emulsion made by mixing the scrapings of agar slant cultures of the organisms commonly found in urinary infections with sterile salt solution, and then injecting it into the bladder of animals. After thorough cleansing of the genitalia, preferably female animals, a No. 4 or 5 (French size) ureteral catheter, such as is employed for röntgenoscopic work, and which can be boiled, was inserted into the bladder, some of the urine taken for cultures, and then an emulsion of bacteria injected into the bladder. All injury to the bladder and ureters was thus avoided, and the conditions under which the organisms migrated upward resembled those found clinically as closely as it was possible to do in the

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animal experiments. Paraffin sections were taken from a number of places in the bladder and kidney, and were studied serially. In order to trace the infection upward along the ureter, every portion of the latter was cut longitudinally as well as serially, so that no areas would be omitted. In five rabbits and eight dogs the *bacillus coli* was injected into the bladder. In three rabbits and four dogs the *staphylococcus aureus* and in three rabbits and three dogs the *proteus vulgaris* were used. In one rabbit an emulsion made by mixing acute gonorrheal pus with salt solution was injected.

Summarizing our results, we find that the inflammatory infiltrations follow in a most accurate manner the course of the lymphatics, as determined by the work of Kumita[2] and Bauereisen[3] (Figs. 1 and 2).

In the early stages of infection, the infiltration is found in the submucous layer of the bladder, and is especially dense around the smaller vessels in this layer. One can then follow the infiltration up into the ureter where, as in the bladder, it is most marked in the submucous coat in the lowermost portion of the ureter. A little farther up, however, in addition to the submucous infiltration, a new factor is introduced. We now see that the periureteral sheath, composed of loose areolar tissue in which many blood-vessels lie, plays an important, if not the chief, part in transmitting the infection upward. Examination of serial sections longitudinally of the entire ureter shows plainly how in the early stages the infiltration around and in the walls of the blood-vessels of the periureteral sheath is the earliest and most constant finding. As the infection progresses, the other coats become invaded from without inward, the mucosa remaining intact, until the infection is well advanced. We find the same sequence

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of changes in the human ureter, on which we shall report in a later paper.

In the kidney pelvis the infiltration is first seen in the subpelvic areolar tissue, and again around the blood-vessels, the overlying mucosa remaining intact (Fig. 3). Within the kidney our experimental work confirms the observation of Mueller[4] in the human being. The infiltration is seen at first around the renal vessels, which pass into the parenchyma to form the arcual vessels, as in Figure 4; then in the cortex the infiltration is seen between the tubules and around the glomeruli, and, as the infection advances, the medulla is invaded in the same intertubular manner, and finally the infiltration breaks into the lumen of the tubule in the more advanced stages. In one experiment, the infiltration followed very closely the lymphatics which allow of communication between the cortex and the lymphatics penetrating the true capsule, which in turn communicate with this perinephritic tissue. We are convinced, as Bauereisen and others have asserted, that there is a free communication between the lymphatics of the bladder and those of the ureter, and that the lymph current is in an upward direction.

Our work shows that the connecting link between the lymphatics of the ureter and those within the kidney is along the lymphatics of the subpelvic areolar tissue which surrounds the blood-vessels as they enter the kidney tissue. They follow within the organ itself the course of the intertubular, periglomerular, and perivascular lymphatics. In six out of twenty-seven experiments, we obtained pure cultures of the same organism from the renal pelvis, which we had injected into the bladder. In none of the animals was a positive culture obtained from the heart's blood, so that hematogenous infection can be excluded.

At the suggestion of Drs. D. J. Davis and E. G.

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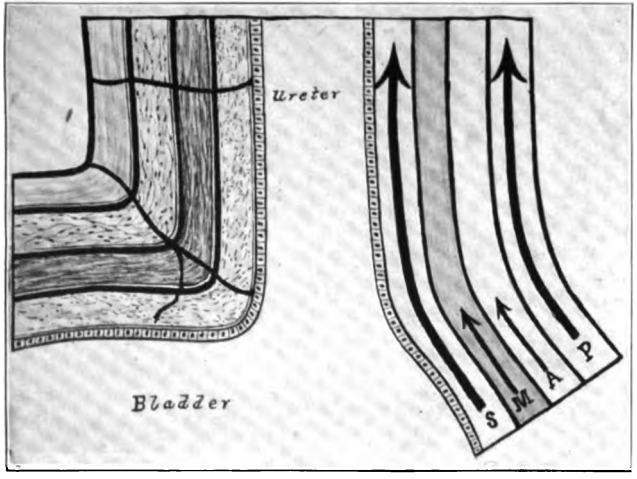

Fig. 1. Diagrammatic view showing how the lymphatics of the bladder communicate with those of the ureter. On the left side are shown in heavy lines the various lymph vessels and how they communicate. On the right side, *S* and *P* represent the chief routes of ascending infection in the submucous and peri-ureteral coats, while *M* and *A* show the less important routes in the muscular and fibrous coats.

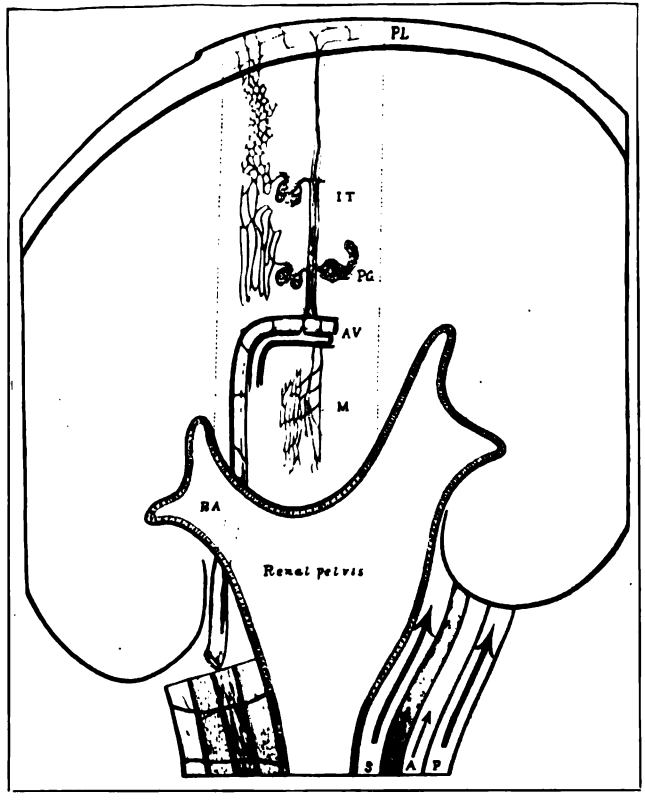

Fig. 2. Routes of infection from the ureter to the renal pelvis and into the kidney. *S, M, A, P,* same as in Figure 1. *RA,* renal vessel lying in subpelvic tissue. *AI,* intrarenal vessels (Kumita) surrounded by lymphatics. The latter communicate with the network between the tubules of the cortex (*IT*) and those of the medulla (*M*). *PG,* periglomerular lymphatic network. *PL,* communication between the intrarenal lymphatics and those of the capsule.

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Kirk, to whom we are indebted for many courtesies, we studied the urinary organs in a number of normal laboratory animals as controls. In six dogs, as carefully sectioned and studied as our injection experiments, we found no evidence of infiltration. If one adds to this number five completely negative dog experiments, we have eleven dogs with absolutely no pathologic changes. In our rabbit controls we have thus far, out of twelve normal animals, found in five of them very slight evidences of infiltration as follows: In one there was a trace of perivascular infiltration around one of the smaller vessels of one kidney out of a large number of sections examined. In a second rabbit there was a very slight, small, round-cell infiltration in the subpelvic and intertubular tissue of one kidney. This was present only in one out of the large number of sections examined. In the other three rabbits there was a very slight degree of infiltration around one of the blood-vessels in the kidney. We have never been able to find any changes in the bladder or ureters of any of the six control dogs or in the twelve control rabbits. In three guinea-pigs, used as a control, we also found no changes in any of the urinary organs. The known susceptibility of rabbits to all kinds of infection, and the fact that the infiltration was present in only a few places in the kidney, and here only where the lymphatics are found, rather confirms our experimental work than otherwise. The existence of a slight degree of ascending urinary infection in these control rabbits is not at all improbable. The relatively large number (eleven) of normal control dogs leaves, at least, experiments in these animals beyond every shadow of doubt, and some of our most positive results were obtained in dogs.

The only other experiments of a similar nature to our own were those of Hess[5] and Bauereisen. Hess

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used *bacillus coli*, and found perivascular infiltrations in the ureter in two dogs. Bauereisen found that, in animals whose bladders were injected with an emulsion of tubercle bacilli, the infiltrations followed the course of the lymphatics from the bladder to the ureter and then invaded the coats of the ureter from without inward.

The many clinical applications of our experimental work along this line will form the subject of a future paper.

REFERENCES

1. Aschoff: Pyelonephritis, Jena, 1893.
2. Kumita: Arch. f. Anat., 1909.
3. Bauereisen: Ztschr. f. gynäk. Urol., ii.
4. Mueller: Arch. f. klin. Chir., xcvii.
5. Hess: Mitt. a. d. Grenzgeb. d. Med. u. Chir., xxvi.

DISCUSSION

DR. EMIL G. BECK (Chicago, Ill.): Dr. Eisendrath kindly invited me to see these specimens in his laboratory; after seeing them I am fully convinced that infection takes place along the outer sheath or through the wall of the ureter, instead of along the inside of the mucous membrane. The question occurs to me that we might try to find out how the microörganisms get through the mucous membrane into the serous layer of the ureter, and whether any lesions could be found on the bladder surface which would show the point of entrance.

Furthermore, I suggested to Dr. Eisendrath to control these experiments by bacteriological tests, to see if he could not cultivate the same microörganisms from the kidney, or, if possible, to find a neighboring gland that was infected by the same microörganism.

DR. EISENDRATH (closing): This work is of more than theoretical interest. In doing kidney surgery I have found that this is something we have to know about, because, otherwise, we could not treat a good many patients intelligently. I have seen clinically a great many times that, if you clean out the bladder, the kidney infection will often stop. It is the same way, for instance, in a case of ascending infection, such as a severe case of pyelitis of pregnancy and of the puerperium which has resisted all other treatment. If you

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wash out the pelvis of the kidney with some solution based on the theory I have enunciated, you can clean up the case.

Then, there is a very important thing pointed out by Sampson in connection with carcinoma, and Bauereisen in connection with ascending infections; and that is the relation between the ureter as it passes through folds of the broad ligament and the gynecological processes, post-operative or pre-operative, so that we can easily understand why these women get infections of the bladder on account of the retrograde lymph-current.

DR. LEWIS L. MCARTHUR (Chicago, Ill.): I would like to ask whether there was a pyelitis in these animals?

DR. EISENDRATH: A simple pyelitis is rare clinically, but almost invariably there is some infection of the kidney; and then you get usually what is known as a pyelonephritis at the same time.

There is one point that has been brought out in a recent article by Chute, and that is this: If you have fever from any urinary infection, it is not from the bladder, but from the kidney, and many times we find in the kidney infections without any localizing sign.

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HOW TO LOCALIZE ACCURATELY FOREIGN BODIES IN THE CHEST, AND METHODS OF THEIR REMOVAL

EMIL G. BECK, M.D., F.A.C.S.

CHICAGO, ILLINOIS

When one suspects a foreign body in the chest, entered either through the bronchus or through the chest-wall, he immediately thinks of the radiogram to locate it, because it is the method least harmful to the patient and most dependable for the surgeon.

A radiogram will show the foreign body, provided the object is dense enough to produce a shadow. Bullets, coins, pin-tacks, and metal buttons show distinctly, but glass, aluminum, and wood are not as easily discernible in a radiogram. If the foreign body shows plainly in the radiogram, we are apt to congratulate ourselves for having already localized it, and, with the greatest of confidence, we decide upon the operation. We believe we have already located the foreign body when we see it in the picture, and all that remains for us to do is to extract it. But, alas! what a disappointment when we cannot find it after searching for an hour or two. This is especially true when it seems so simple,—for instance, a needle in the hand, where the field of operation is so small and the needle shows clearly in the picture.

"Are not these radiograms deceptive?" we exclaim. Does the radiogram deceive, or is it possible that it is

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our own fault that we cannot find the foreign body? The radiogram does not lie: it obeys certain physical laws. The shadows have been produced by dense bodies, no matter whether we can find these objects or not. The fault is usually our own. We are apt to interpret the shadows incorrectly, or we may fail to find the foreign body in the field of operation, often when we have our fingers on it.

Discouraged, as we were, with some of our failures in locating foreign bodies years ago, I began to search for the cause, and soon discovered that it was the faulty interpretation of the single radiogram, which was constantly misleading us.

A shadow produced by a foreign body does not always appear in the same place on a single picture. Its location depends upon the angle at which the focus of the *x*-ray tube is placed at the time of exposure, as illustrated in the following experiment.

Figure 1. I placed a bullet on the top of a young girl's chest, right in the center of the sternum, and then took two radiograms without changing the position of the bullet or of the patient, but shifting the tube between the two exposures. On each radiogram the shadow of the bullet will be found in a different place. In one picture it is located on the left side within the lung area; in the other, in the center of the spine. Both interpretations are incorrect, as we know the bullet is on the sternum.

Examine the single plate, and we find all the shadows are in one plane, one shadow overlapping the other, so that we have a blurred composition of superimposed shadows, one obliterating the outline or contour of the other. Besides, the size of each shadow is enlarged, and its density diminished, just in proportion to the distance of the object radiographed from the sensitive plate. Such discrepancies must, of

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necessity, make interpretations very difficult and lead to many errors.

One can recognize a fracture by the single plate, of course, but he cannot tell the angle of displacement of the fragments. One can see a foreign body in the chest or in the skull, but he cannot tell positively how far it is from the surface, or whether it lies anteriorly or posteriorly. This proves the fallacy of trying interpretation of single radiograms with any assurance of a correct anatomical diagnosis.

To overcome this fallacy I have employed almost exclusively during the past eight years stereoscopic radiograms, instead of single pictures, for locating foreign bodies. My brothers and I have not had a single case in which we did not localize the foreign body since we have employed this method.

As you know, the stereoscopic method consists in taking two pictures of the same body from two different angles, just as the eyes do in inspecting an object. The two radiograms when dry are placed in a stereoscope, and a single plastic picture is thus viewed, which gives us, not only the height and width of the object radiographed, but also its depth. In other words, we get all three dimensions; whereas, in the single plate we obtain only two dimensions,—height and width. Here we obtain a plastic picture, in which the distance between the different objects is easily estimated, and the structures which are radiographed appear more concrete and distinctly outlined.

One obtains an entirely new conception of radiographic work when he has employed the stereoscopic method, especially for locating foreign bodies. It is easier to convey this information by actual illustration than by description, and I present here some of my radiograms reduced to the parlor stereoscope size, and, although the detail is lost to a great extent by this

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reduction, yet enough can be seen to convince you of the great advantages of this method.

I shall cite in detail seven instructive cases in which I employed this method, and I believe that, if you get into the habit of using it to localize foreign bodies, you will find it of such advantage to yourself and to your patients that you will adopt it for diagnostic means in surgery and medicine.

Many suppose that the stereoscopic method of taking radiograms is complicated. This is an error; it is just as simple as taking single pictures. Others hesitate to use it because they believe it is more expensive. Usually it requires two single pictures for an ordinary diagnosis,—a lateral and an anterior-posterior. Thus the expense is about the same, and the two single radiograms will not give nearly as much information as one stereoscopic set.

In order to increase the accuracy and usefulness of localizing foreign bodies, I have devised another adjunct, namely, a small wire screen localizer, which I place in the region where I suspect the object to be, and fasten it to the skin with adhesive plaster. The shadow of this screen helps to estimate the distance between the skin and the foreign object within the chest. Sometimes I place another small wire screen on the opposite side of the chest, and thus I can easily estimate the relative distance of the object within the chest from both the anterior and posterior surfaces. Here is an illustration:

CASE 1.—A physician from Oklahoma had an abscess in the lung, draining externally for six years. In order to localize the abscess cavity, I introduced a catheter through the sinus-opening, and, after siphoning off about four ounces of pus, I injected through the catheter a small amount of bismuth paste and left the catheter in situ. A small wire screen was then placed on the back in the region where the

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abscess cavity was suspected and a stereoradiogram was then taken.

Please view the stereoscopic radiogram (Fig. 3), and note the relative distances between the catheter (A), the bismuth (B), an empty cavity (C), the ribs (D), and the screen on the skin (E). You should be able to figure out approximately the number of centimeters intervening between the different objects.

Thus, if I should have to drain this abscess, I would know exactly where to make my incision and what ribs to resect. From a single picture alone I do not see how I could tell with certainty whether to make the incision anteriorly or posteriorly. In fact, the surgeons who originally drained this abscess made their incision in the axilla, about six inches from the most desirable location for drainage.

I did not have to operate on this case at all. Closure of sinus after first injection, and a gain of thirty pounds since July, 1915, allow me to assume that an operation will not be necessary.

The second case illustrates the localization of a bullet and its extraction in a case where the single picture would surely have misled us.

CASE 2.—A young woman tried to commit suicide by shooting a 22-caliber bullet through her heart. She was brought to the hospital immediately after the occurrence, and there was nothing externally visible except a small bleeding wound between the second and third ribs to the left of the sternum.

A stereoscopic radiogram was immediately taken. The bullet in the single picture appears to be in the axilla (Fig. 4). The stereoscopic radiogram, however, dispels this optical delusion, and proves that the bullet has penetrated the chest-cavity and landed against the scapula, the impact producing a mushroom bullet.

Thus we know that the missile is outside the chest-cavity, anterior to the scapula. A small incision under cocaine, and the chiseling away of an inch of the thin plate of the scapula revealed the bullet, which was extracted. The whole procedure took only ten minutes.

CASE 3.—A young woman from Cuba inhaled a pin about two inches long some four years ago. The matter was entirely forgotten until during this past year, when she began

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to expectorate blood. A diagnosis of tuberculosis was made, and she was sent to California. Her physician there took a radiogram, which disclosed the presence of a pin in her right lung. He brought her to Chicago to my brother, Dr. Joseph Beck, for extraction of this pin by means of the bronchoscope.

In consultation with two other specialists in this line, we pronounced the case to be unsuitable for bronchoscopic extraction, and it was turned over to me for external operation. The stereoscopic picture gives me full information as to the location of the pin and its direction. I was able to remove it by resecting about two inches of the tenth rib, and had no difficulty whatever in locating the pin as soon as I entered the lung. The procedure was so simplified and the duration of the operation so shortened that we lost no time in locating the pin during the operation. Recovery with complications followed. Expectoration of blood ceased (Fig. 5d).

CASE 4.—A young Servian was shot some fourteen years ago; and, after a dozen buckshots were removed from his body, two remained in his back and caused him much discomfort.

A single radiogram shows the two buckshots but we are unable to say how deeply they are lodged. With the aid of a stereoscopic set and the use of the wire screen localizer I could ascertain that one was anterior to the eleventh rib, the other in the transverse process of the seventh dorsal vertebra.

The operation proved the correctness of our interpretation of the stereoröntgenogram. Both buckshots were easily located and extracted (Fig. 5e).

CASE 5.—A deaf and dumb boy, about fourteen years old, was operated on two and a half years previously for an abscess in the lung. After a short period of drainage the wound closed, and he was constantly ill, and had pain in his chest. I took a single radiogram in this case, and noticed that there was a dark shadow on the sixth rib. This shadow coincided with the center of the rib, so that I interpreted it as a tubercular rib.

I decided upon a resection of that rib. Upon incision I found its outer surface entirely clean and healthy, and when I resected the part which was supposed to be involved, I found

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it absolutely normal. I punctured the pleura, and found an abscess underneath this point.

Upon evacuating four ounces of dark, thick pus, I found two pieces of rubber tubing within the cavity, which are shown in the photograph (Fig. 5r). These two pieces of rubber tubing had evidently slipped into the abscess cavity subsequent to the first operation, and in spite of this the opening closed with the rubber tubing within the cavity. The single radiogram deceived us. The tubes happened to lie in the same plane of the 6th rib, and produced a superimposed shadow, which made it appear that the 6th rib was diseased. Had I taken a set of stereoscopic radiograms I would very likely have distinguished the two separate bodies,—rib and rubber tube. As it was, the two shadows,—one caused by the tube and the other by the rib,—were superimposed, and the blurring of the two shadows produced a picture which one could not interpret unless he knew beforehand that there was a rubber tube in the cavity.

This case teaches us that the single radiogram in locating foreign bodies is not reliable.

Case 6.—E. D. At fifteen the family history is negative as to tuberculosis. Was well until four years ago, when he inhaled a brass pin-tack, the size of a lentil. Within a week he developed pneumonia. After the acute symptoms passed he continued to expectorate pus, and had a daily elevation of temperature from one-half to three degrees. In spite of all medical and climatic treatment, the above symptoms persisted for four years.

I first saw him December 15, 1908. His weight was 98 lbs.; temperature, 97°; pulse, 120; cyanotic and greatly distressed. The entire chest was resonant, except the right posterior and lower part, the dullness extending upward to the angle of the scapula. Stereoradiograms were taken, which were of great assistance in the anatomic diagnosis, since they brought out, in plastic effect, the location of the abscess and its relation to the other structures and also the foreign body.

The expectoration had a fetid odor, was green in color, and at each coughing spell at least half an ounce of pus was expectorated. Microscopic examination of the pus shows streptococci, pneumococci, and staphylococci. An operation was was decided upon in this case, and a modified method of treating abscess of the lung employed, which I shall report in

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detail in another communication on abscesses of the lung and their treatment.

The pin-tack was removed (Fig. 5c), and the abscess cavity obliterated with a large skin-flap.

The young man is here exhibited, and you can inspect the abscess cavity with either a head mirror or a rectoscope with direct illumination. Two of the bronchial openings have already closed, and the discharge has diminished daily. The inserted flap has entirely grown on and covers the inner half of the entire abscess cavity.

CASE 7.—This case exemplifies a foreign body in the lung which does not show in the röntgenogram, illustrated in the following:

A young man at twenty-four developed an empyema four years ago. The cavity was drained as usual, but after a reasonable length of time it failed to stop discharging. The physician in charge tried to paint the walls of the abscess cavity with iodine, and took a long wooden probe with a pledget of cotton dipped in iodine and applied it internally through the drainage opening. The end of this wooden probe broke, and in spite of all search the doctor could not find it in the cavity. Two operations were performed for its extraction, but in neither of these operations could the surgeons find the wooden probe. The cavity was then drained by a large rubber tube for two years.

I removed the rubber tubing, injected the sinus with bismuth paste, and obtained a peculiar picture (Fig. 6). It shows the cavity contracted to a channel, occupied by the rubber tube, and one can see distinctly the shadows produced by the granulations which had grown into the holes of the rubber tube.

I did not re-insert the rubber tubing, and, behold, next morning the end of the probe was found in the dressing (Fig. 5b). The cavity closed up within a week, and has been closed for the past four years.

This last case does not aid us in localizing foreign bodies, but it illustrates that foreign bodies may be present which do not give any shadows in the single picture nor in the stereoscopic.

The single radiogram is instructive so far as it shows the size of the existing cavity and also its direction.

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In conclusion: One might infer from the report of these cases that I regard the external operation as the only means of extracting foreign bodies. This would be an error. The extraction of inhaled objects, such as coins, buttons, etc., by means of the bronchoscope is much to be preferred to operation, provided it is feasible. At the North Chicago Hospital we have had a series of such cases in which the foreign body was extracted through the mouth, but in a certain percentage of cases we were obliged to resort to the external operation; the bronchoscope could not expose the objects.

I wish I could now present this subject in the large military hospitals of Europe, because a reliable method of localizing shrapnel in the chest, or in other parts of the body, must at the present time be to them a welcome aid. Although it is not necessary in every case to extract a bullet as soon as one is shot, the knowledge of its location is always necessary.

I do not wish to convey the impression that the stereoradiogram is the only means of localizing foreign bodies. We have to consider also the fluoroscope, and I believe that it is preferable to the single radiogram. It does not give us a permanent record, however, and the surgeon who is to extract the foreign body is usually not a master in the use of the fluoroscope. Thus, since he cannot study the case very long under the fluoroscope, he will be much better prepared to study stereoscopic radiograms.

At the North Chicago Hospital we have used this method exclusively since I introduced it, in 1907. I am pleased to say it has now been adopted in practically all large hospitals in this country and in very many hospitals abroad, where I demonstrated and published this method five years ago. We employ it also in intestinal work, in studying the diseases in

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accessory sinuses of the head, in estimating cavities in empyema, in tracing sinuses, in fractures and dislocations, and in the diagnosis of pulmonary tuberculosis (in the incipient as well as in the post-tubercular stage). In fact, we have given up the single radiograms and believe that all those who are progressive in this work will do likewise.

REFERENCES

[1] Emil G. Beck, M.D.: Stereoscopic-Radiograph as an Aid to the Surgeon. (Surgery, Gynecology and Obstetrics, 1911.)
[2] Emil G. Beck, M.D.: Die Stereoskopische Radiographie in der Chirurgie; ihre Vorteile gegenüber dem einfachen Radiogram.

DISCUSSION

DR. L. W. LITTIG (Davenport, Iowa): I have very little to add to Dr. Beck's paper. He has presented the advantages of the stereoscopic method for locating foreign bodies. At the present time this method is endorsed abroad very highly. In a recent discussion before the Royal Society of Medicine in London, one of the speakers, while admitting that the ordinary string method and the triangulation method had advantages, yet considered the stereoscopic method incomparably superior. The pictures must be taken just before the operation, because these foreign bodies migrate. I think this statement made by one of the speakers before the Royal Society of Medicine of London is a strong endorsement of the position taken by Dr. Beck. The same speaker recommends, likewise, that in future wars a stereoscopic screen method be developed, and this will be the method superior to all others.

There is one point in connection with the subject which I think is immensely important, and that is to remember that foreign bodies after they have passed the mouth, very frequently are followed by a period of calm and quiet. There are many cases that come to the post-mortem table with a diagnosis of tuberculosis, or abscess of the lung, in which röntgenoscopic examination was not made, and in which the diagnosis was not made because of the elusive and deceptive period of calm. Whenever lung trouble of any kind is discovered, without much, if any, disturbance following the swallowing of a foreign body, examination with the radioscope must be made. We know that children not infrequently swallow coins, and these coins remain in the esophagus for

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weeks and months without causing any disturbance of deglutition, the stomach-tube being passed without difficulty. All this, it seems to me, adds strength to the contention of Dr. Beck that the stereoscopic method is the method of choice, and should be resorted to whenever there is any reason to suspect a foreign body.

Dr. Arnold Schwyzer (St. Paul, Minn.): I suppose the stereoscopic method is generally the best for locating foreign bodies; but, as Dr. Beck chose the general subject of how to locate a foreign body, we should speak also of the radiographic methods.

If you have a foreign body somewhere in a bronchus low down, and you do not get good pictures because it is a kernel or a seed, you still have to go after it. I remember very well the case of an individual who told me definitely the foreign body was on the left side, but there was a small area on the right side which would give some moist bronchial râles. So we felt sure the foreign body was on the right side, notwithstanding what the patient claimed. We made a tracheotomy, introduced the bronchoscope, went down to the bifurcation, went into the left lung, and found the main bronchus and the secondary bronchi normal. They were pale. As soon as we turned to the right side we found the bronchial tube was red, and contained mucus. We followed the main bronchus down on the right side, and found the foreign body, so that the secretion, redness, and swelling of the mucosa, seen in the bronchoscope, served as a guide in enabling us to find the foreign body, and remove it.

Dr. Charles H. Mayo (Rochester, Minn.): The stereoscopic method, which Dr. Beck has done so much to develop, is quite generally used, but is particularly valuable for chest-observation.

To those of us who did surgery of the lung years ago, before we had the benefit of the *x*-ray, it was not at all uncommon to have a child brought in who had put some foreign body in its mouth and inspired it, and then nearly died of strangulation. By searching and listening for crepitation in a certain portion of the lung, we were able to localize the foreign body, and remove it by tracheotomy. Later on, when we did get the benefits of localization by even a single *x*-ray plate or more, they would show a shadow of some things to distinct advantage. But the question that comes up is where

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to draw the line for surgery in these cases. The bronchoscope will often enable one to pick out a foreign body in a bronchus. I recall one instance in which a long pin remained in place for four years without symptoms. Tacks and nails have remained in the lung a short time, and abscesses have formed. When an abscess forms, external operation with drainage is the best treatment.

I have seen Dr. Plummer remove a foreign body by means of the bronchoscope after it had remained four years, notwithstanding two unsuccessful surgical operations had been made to locate it. These foreign bodies are not so easy to locate as they appear, even after a fluoroscopic picture is obtained.

Dr. L. L. McArthur (Chicago, Ill.): I think the case shown by Dr. Beck contains the meat in the cocoanut,—that is, many of these chronic abscesses, called frequently chronic empyemata, for which resection of the ribs and collapsing of the wall are made without success, have some hope of being cured now by his method of marsupialization of the abscess cavity, by which he can see the openings of the bronchi into the abscess cavity. By destroying their mucous membrane later the granulation occludes them, tissue occludes them, and the abscess cavity is cured premanently.

Dr. William Fuller, (Chicago, Ill.): I would like to ask Dr. Beck to tell us, when he closes the discussion, the object of implanting skin so deeply into the chest-cavity. This tunnel of skin appears to go down several inches. In the final closure of the wound what becomes of this tunnel of skin?

Dr. Miles F. Porter (Fort Wayne, Ind.): It seems to me in a bronchus of the size shown in that cavity, to destroy the mucosa of that bronchus would not do any good at all unless there was destroyed, at the same time, the cartilaginous ring of the bronchus also.

I would like to ask Dr. Beck whether he destroyed that, as well as the mucosa?

Dr. Beck (closing): Answering the question of Dr. Fuller: The reason why I implant skin so deeply is that I want to keep the cavity open for treatment; otherwise it would close too soon, and even the insertion of a tube would not keep it open.

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As to what becomes of that wound: There will be a deep dimple of the skin. After I destroy the mucous membrane of these bronchi, they will close up, and then I will allow the cavity to shrink. It has shrunken about half now, but I am still trying to keep the cavity open.

In answer to the question of Dr. Porter in reference to the ring preventing the closure of these small bronchi: I say it will not interfere with closure. There is not a very solid ring there. It is composed mostly of connective tissue, very little cartilage. The fact is, these cavities do close. In this case two out of seven have already closed, and they would close even if they were three times the size. Such bronchi close in empyemata where we have a large bronchial fistula.

The inspection of these abscess cavities by this method throws a new light on the character of these lung abscesses. Formerly I had the idea that these lung abscesses consisted of a uniform globular sac, but this case, and several others which I treated similarly, teach me that these abscess cavities are multilocular, the compartments being formed by septæ some of them quite distinct and thick. In this case I can grasp one of these septæ between my fingers, and feel the pulsation of a good-sized artery; other septæ are thin, translucent, somewhat like the web of a frog's foot.

In the bottom of each compartment is a bronchial opening, sometimes two. These septæ must be divided, and this I shall do by crushing them, by applying artery forceps for a day or two, so as to avoid bleeding.

I described this method of treatment of abscesses some four years ago, but never used the implantation of the skin. I have cauterized the bronchi in this manner, and they have closed.

The objection raised that the cartilage ring will prevent the closing is theoretical, but practically it does not prevent it.

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TREATMENT OF SHOCK WITH EPINEPHRIN AND TRANSFUSION

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MINNEAPOLIS, MINNESOTA

The prophylaxis of shock has of late years received so much consideration, and the principles for the prevention of shock have been so generally practiced, that the post-operative occurrence of this condition is rather rare. I have made careful study of all cases at the University Hospital for a period of three months. In but three of these was there any marked evidence of shock, and these cases were very desperate surgical risks. In all of the others there was only very slight change in the blood-pressure tracings throughout their stay in the Hospital. Some cases showed slight post-operative rise of blood-pressure, and others a slight fall. There was usually about one degree of post-operative fever and a slight increase in the pulse-rate.

In the series was one case where a man had been kept under ether for six hours for a very tedious brain operation. This patient showed a rise of blood-pressure when first put under the anesthetic and only a very slight fall toward the close of the operation. This final blood-pressure was 10 mm. higher than when the patient went to the anesthetizing-room. There was also in the series one case of fairly well-marked acidosis, as indicated by the urinary examinations. This man showed only a rise of blood-pressure following gastrostomy. In the three cases there was

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very marked disturbance following operation, evidenced by blood-pressure records and the usual clinical signs of shock.

The term "shock" is used with a great variety of meaning. Some authors consider any disturbance of blood-pressure as shock. When I spoke of the symptoms of shock I meant the fully developed condition, as evidenced by pallor of mucous membranes, by weak pulse, greatly lowered blood-pressure, loss of sensibility, as shown by the lack of necessity for administering ether when the eyelid reflex was present, and irregular, rapid, shallow respiration. This description is paraphrased from Mann's description of physiological shock. When this picture is present, some radical treatment must be resorted to to save the patient. For this reason my paper might be more properly entitled "the radical treatment of shock."

Collapse sometimes occurs in the course of shock, as I have often seen a sudden decline in blood-pressure in an animal not well advanced in shock, that could be attributed only to collapse. Therefore the clinical cases I am about to report may possibly include both shock and collapse. Whether these are pure cases of shock or mixed cases of shock and collapse, is a question that could be answered only on academic grounds.

Our knowledge concerning the treatment of such conditions does not seem to be in a very satisfactory state; therefore I shall detail the treatment of these cases, and corroborate my observations with a much larger series of animal experiments that imitate, as far as possible, the clinical conditions.

Before doing this, I shall review what has been done for the treatment of actual shock. Acting on the supposition that the low blood-pressure in shock was due to vasodilatation, clinicians formerly used normal salt solution either intravenously or by the

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rectum. I have seen two clinical cases do very badly on this treatment, and experimentally I have corroborated the findings of Crile, that the intravenous administration of salt solution is of more than doubtful value in shock, in that the solution may be speedily lost from the vessels into the tissues. In some instances death ensued from waterlogging the lungs; in other cases, the primary rise of blood-pressure was followed by a secondary, more severe fall. This, in the experimental laboratory, was due to escape of fluid into the free peritoneal cavity. I have killed animals in shock repeatedly with intravenous saline solutions; therefore, the use of salt solutions intravenously seems to be dangerous, and in advanced shock normal salt solution in the bowel is oftentimes not absorbed.

Second: The attempt to re-establish the blood-pressure by pneumatic suits never has seemed to merit a general trial. From a purely theoretical standpoint it hardly seems possible to attain vasoconstriction by pressure over the outside of the body.

Third: The use of adrenalin in one to one hundred thousand dilution was at one time extensively tried. In some instances this procedure has done harm, probably on account of the improper method of administration. The intravenous administration of adrenalin should always be governed by blood-pressure observations. When this is not done we may administer an overdose, and this overdose damages the blood-vessels and may increase shock. Further, the administration must continue for a long time as the effect is transitory.

Fourth: In transfusion we have a remedy that seems to benefit surgical shock, but in some instances transfusion fails, as it recently did in one of my own cases. On account of this, I reduced a number of

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dogs to shock and transfused them. Usually the results were good, but in some instances the blood acted exactly like normal salt solution,—that is, it was lost in the perivascular tissues. Two of these animals where this accident occurred showed very marked depletion of the epinephrin content of the suprarenal gland. This suprarenal depletion I believe is common in shock. In a physiologic study that I have reported elsewhere I came to the conclusion that epinephrin was an agent in overcoming shock, and that shock developed when epinephrin was not available for action on the blood-vessels. The reasons for this assumption were, first, that animals reduced to shock showed suprarenal depletion; second, that animals deprived of suprarenals rapidly went into shock; and, third, that the usual adjuvants to shock, such as ether narcosis and infection, were also important agents in reducing the epinephrin content of the suprarenal gland. In ordinary traumatic shock it is claimed—and I believe it to be true—we have a vasoconstriction of all vessels. In addition, I also have reason for belief that there is an actual loss of blood volume, as has been indicated by the work of Lyon and by Sheerington, Copeman and Mann. When vasoconstriction fails to keep pace with diminished volume of blood, we might expect a fall of blood-pressure and finally shock. If there is loss of volume in shock there is also loss of function on the part of the blood-vessel walls, allowing escape of fluids. Henderson's teachings, together with Cobbett and Sheerington, corroborate this belief; therefore, if these principles are true in shock, when the central nervous system is excluded from the pathology, it seems probable that we could get a cure by doing two things, first, by supplying the vasoconstrictor principle, and then by supplying more blood. Twelve experiments on dogs confirmed this

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belief. When we took an animal and reduced him to shock we found that there was a constantly increasing amount of fluid loss in the free peritoneal cavity and probably as much more into the tissues. When we administered epinephrin to such an animal this escape of fluid was retarded, although the blood-pressure was thereby increased. To maintain this, epinephrin had to be continuously administered, and inasmuch as this interfered with the function of the kidneys it did not seem an advisable thing to continue indefinitely; therefore, the blood volume was next increased by means of transfusion.

Transfusion after epinephrin did not increase the escape of fluid into the peritoneal cavity, but did increase the flow of urine.

It is of interest to note that the transudates recovered from the peritoneal cavity were practically the same hydrogen-ion concentration as the blood, and, further, that these transudates were not influenced by sodium bicarbonate injections. With this foundation I attempted to make application of this method of treatment to these cases of shock following an extensive abdominal operation. This is typified by the following case:

The man before operation was in bad shape but had a fair blood-pressure. On the table, without the occurrence of any hemorrhage, he developed an extreme picture of shock. His blood-pressure fell to 40 mm. after gradual decline. This was at once combated by the use of intravenous epinephrin. Just enough of the solution was continuously administered to maintain blood-pressure of as near 90 as we could. After two hours and a half of this the epinephrin was discontinued. The blood-pressure soon fell to about 70, and remained there. On account of embarrassed respiration and anuria, transfusion was done eleven hours after his operation. This resulted in a permanently improved blood-pressure without any more adrenalin, and resumption of urine and other functions.

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I have attempted to explain the improved blood-pressure that continued for some time following the continuous administration of epinephrin on the following ground: In the experimental laboratory we have found that the suprarenal gland seemed to recover its function following the administration of intravenous epinephrin; therefore, I think this combination is a good one: first, using epinephrin carefully, controlling the administration of this with blood-pressure observations, and finally completing the work with the transfusion. This has the advantage of practicability, as the administration of the epinephrin does not in any way interfere with the operator, and time is allowed for arranging the transfusion.

A word of warning must be given. Should the collapse be due to myocardial disease, the use of epinephrin is contra-indicated on account of the undue strain it will throw upon the heart. These cases, if treated at all, should be approached from the standpoint of heart-therapy.

The remarks I have made concerning shock apply to the common type we see in traumatic and post-operative cases, as well as to their prototype in the experimental laboratory. Exceptional cases have been recorded that do not seem to be explainable on these grounds. These cases of inhibition, collapse, or disturbance of nerve centers, cannot be included in this description of shock, and must be approached from a different angle. Langley's conception of the autonomic and sympathetic control may offer some suggestions, but as yet we can hardly apply them.

DISCUSSION

DR. ARTHUR E. HERTZLER (Kansas City, Mo.): So far as I can see, the problems that present the greatest difficulties are, first, that we do not know much of what blood-pressure means, and, secondly, we know little or nothing about the

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action of epinephrin and its physiology. I have been trying to find out what blood-pressure means relative to the risk in surgical operations; and, if there is any guide, I have been unable to find it.

With reference to getting blood-pressure: Individuals will differ as to the blood-pressure in certain cases, and I have come to the point of having the same man take all blood-pressure readings. A patient with a low blood-pressure may go through an operation without difficulty, and again he may show a tendency to shock. The relation between pressure and the type of operation, the circumstances of operation, the anesthetic, etc., have to be considered in finding a way out of what is now a hopeless muddle. In studying these conditions with the use of epinephrin, as one does in local anesthesia in using it combined with novocain, a few things stand out rather prominently. The chief one that is universally positive is the sensitiveness of exophthalmic goiter patients to epinephrin. They respond very readily and consistently to it. Now, in all these patients there is a stage of excitement, which is more prominent in Basedow patients than any other condition. Dr. Harris thinks this state of excitement is purely a neurotic condition, but I do not believe it is. I think it is due to the action of the epinephrin.

Dr. Major G. Seelig (St. Louis, Mo.): Dr. Harris in speaking of the difficulty in estimating the physiologic dosage of epinephrin or adrenalin, has suggested a phase of the question that requires great emphasis. It is the difficulty of determining the physiologic action that makes the use of this drug dangerous at best, and particularly dangerous when administered in the face of a falling blood-pressure, such as we get in shock. I have determined frequently in the laboratory that epinephrin administered most carefully will produce results similar to those outlined in Dr. Corbett's chart. For instance, let this (indicating) represent our base line. The animal goes into shock, and the shock level is established at that point. You administer adrenalin or epinephrin, and you get a rise if you administer it for a long time. You will sustain the rise as long as you administer it, if you administer it carefully, but when you cease its administration you get a reaction, and the level is established at a lower point than where it was when you started the administration of the adrenalin. I am making that assertion because no less a pharmacologist than Dr. Jackson assures me that repeatedly he has gotten exactly the same

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effect, so it is fatuous to argue that by giving it in small quantities and holding the pressure at a certain point you are all right.

Dr. Corbett did not emphasize the point sufficiently that, when the administration ceases, this low level here (indicating) is a good deal lower than was the level of pressure before the administration of adrenalin was started.

Dr. Henry T. Byford (Chicago, Ill.): It occurs to me that the more I hear about shock, the less I know about it. From a clinical point of view, shock may be considered as a sudden or rapid recession of the blood from the nerve centers. Clinically, I take this as a working basis, and I find I can work out my treatment from that better than in any other way. The simplest kind of case from this point of view would be hemorrhage. Another kind of case would be one in which the patient has had a large tumor removed from the abdomen and there is a rush of blood into the abdominal viscera, so that the supply to the nerve centers fails. Another kind of case is where a woman gets frightened and faints; and still another kind is where an over-dose of the anesthetic has been given and the blood leaves the nerve centers. Another kind is where a patient faints without shock. In the three last-named cases there is probably a more or less prolonged and more or less complete failure of the heart to supply the cerebral circulation.

With reference to the remedies: Before considering the use of epinephrin and the blood-pressure in general, one ought to consider what is the matter. Of course, in cases of loss of blood we can transfuse, and in a case where there is so-called bleeding in the abdominal viscera, if we cannot raise sufficiently the blood-pressure in the abdomen, we can elevate the foot of the bed, and thus get some blood back into the nerve centers. The whole principle is to get blood back into the nerve centers, wrap the limbs, lower the head and shoulders, do a transfusion, and give epinephrin or any agent that will increase blood-pressure and send blood back to the nerve centers. Large doses of epinephrin are usually contra-indicated, because their influence does not last long enough. A small physiologic dose must be given frequently, so that their action will be continuous until the volume of the general circulation has been restored or the heart has resumed its function.

Dr. M. L. Harris (Chicago, Ill.): The reader of this paper called attention to the necessity of controlling the administra-

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tion of epinephrin by taking the blood-pressure, and to the fact that epinephrin was very transitory in its action. It has to be given continuously and in small amounts. These points, I think, have not been sufficiently emphasized. They are the most essential parts of this paper in my opinion. Epinephrin in physiologic quantities, we believe, has a beneficial influence in maintaining normal blood-pressure, but, when epinephrin is increased beyond the physiologic amount, it becomes exceedingly toxic and very dangerous. You can kill an animal with epinephrin; and you can put a patient in serious collapse in a very few minutes by the injection of excessive doses of epinephrin. I have seen patients on the table pass into a serious state of collapse, with an exceedingly weak, slow pulse, dyspnea, and great pallor in a very few minutes after the injection of epinephrin. So I wish to emphasize most emphatically the dangers of the indiscriminate injections of epinephrin. In a state of collapse it should be administered with great care. The effect on the pulse and blood-pressure, to which the doctor has called our attention, must be carefully watched to get any good out of it in a state of shock. It must be administered for a long time in very small amounts to supplement the physiological amount, because experiments have shown that if we exceed the physiologic amount we may injure the patient and may even increase the shock, and the patient be in a worse state than before.

Dr. Joseph C. Bloodgood (Baltimore, Md.): I thank you very much for this opportunity to say a few words. I have just written some of the members of the Western Surgical Association a letter in regard to shock, sending them a reprint by Dr. Carman of the Mayo Clinic, who represents another view of shock, and there is a short note of my own which practically covers the discussion of this paper.

I did this because I felt that the recent laboratory work on shock that has been done has created such a dilemma in the minds of practical surgeons, that, unless something is done quickly to find out where we really were, perhaps in the next few years our results in shock will be worse than they were before we had any laboratory suggestions as to what to do. That does not mean any criticism of the work of laboratory men. Acting in medicine somewhat on general principles, today, when new things come up, we give up the old things that have been established before the new things have been established. Meltzer said that before the American Associa-

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tion of Physicians this year. He felt that one of the things we are attaining that should be discouraged was the giving up of the old methods of treatment to take on something new. I will not go into the details of that, but the question is something like when you are building a new bridge it is a good plan to stay on the old bridge that has carried you over for many years, and not cut down the old bridge until the new bridge is thoroughly tested.

I believe that all surgeons have a clear idea that there is such a thing as shock. They may not be able to define it, but they look upon it as a thing that is real; and surgeons who recognize shock before they must operate, or after or during the operation, have some pretty definite conclusions as to what to do, and I believe that, although you may not agree with Crile, he has at least given us a better conception of shock and given us certain definite things we can do.

Now, I have been using blood-pressure records in the cases operated upon for the last five years in every case. It does not make any difference whether we are going to operate under novocain, or whether it is a small operation, we take the blood-pressure record, and I am willing to place myself on record, I have put it in the literature, that the blood-pressure record, taken rather inaccurately by a nurse, will be an assistance to a surgeon after he has used it six months or a year. It will be only an additional assistance to his interpretation of the condition of the patient—additional to what he has already learned in regard to estimating shock.

Crile has shown that the best treatment of shock is similar to the best thing to do in all things when you do not want to get into a hole. The thing to do is not to have shock. I think that is thoroughly understood. You have to bear in mind what shock is and what produces it. In a large number of operations you cannot get the patient in a condition of shock for which we at the present time have no definite treatment. Then again, your keen observation as an operator makes you feel what you are doing is one which will produce shock, and where the pulse record and breathing of the patient show what is going on, it is much better, if possible, at that time to stop those things that you know are producing shock rather than to use any therapeutic treatment for shock.

We know that any anesthetic produces shock; therefore, if possible, you can stop the general anesthetic which in the great majority of cases is ether. You can stop the operation, and do

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it in more than one stage. If you have loss of blood, which in the majority of cases of operative surgery today is not one of practical shock, you can use transfusion. Before giving any drug you can try some of the things that have been mentioned. The one next to the diminution of the ether and trying to stop the operation is lowering the head of the patient; and then you can give salt solution subcutaneously. You can give intravenously salt solution or blood-transfusion, but in my experience the last thing to do is to give any drug, such as digitalin and strophanthin intravenously, or the intravenous continuous dosage of adrenalin, or whatever you want to call it. According to the literature, that has been done for over ten years with experimental work on dogs with just the same results we hear about today. We do not know the dosage. It is uncertain. It may raise blood-pressure, but there is a fall afterwards. From my experience, when a patient is in shock it is better not to raise the blood-pressure with any drug, but to stop the things that are lowering blood-pressure first. Sometimes in a great emergency where an operation must be finished, we may find this drug is the only thing we can give that may add to the possibility of the patient surviving those few moments while you have to continue to produce shock with the anesthetic and with the operation. In the last year I have not used any drug in any case of shock, and I feel pretty certain that I have not lost any cases from not using drugs, and I am very skeptical in regard to the cases previous to a year ago where I used strophanthin intravenously, that it had anything to do with their recovery. I have not been able to persuade myself to use continuous intravenous adrenalin or any of its preparations because of my familiarity with this work upon animals. I do not believe that the rise in blood-pressure is always the thing you ought to strive after in a case of shock, because it has been shown here that after you stop the drug the sudden fall in blood-pressure is a more dangerous thing than is a low level previously, or before you gave it. I do not think we know very much at the present time about the therapeutic employment of any drug in shock; and perhaps Dr. Corbett has brought before us something that will be of great value. In these cases we must continue the operation and the use of the anesthetic, and when the blood-pressure has reached a point that is dangerous, if we do not do something, the patient will not survive. However, those cases

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are very few; they are so few that you are counting they will never get on the operating table.

Dr. W. W. Grant (Denver, Colo.): Dr. Hertzler has correctly stated the confusion and want of knowledge as to the meaning of shock in relation to blood-pressure. But I think the average surgeon understands when he sees a severe case of shock it is shock, whether he understands the physiology of it or not. The conflict between physiologists, like Henderson and Porter, and Crile among the surgeons, as to the meaning of shock is still unsettled. We do not know what it means, but we do know the patient is in a condition called "shock."

I would like Dr. Corbett to explain why, in a case of acute shock with no hemorrhage and no loss of blood serum, we should fill the vessels with more blood and serum, and produce the unfavorable results which sometimes follow. Blood-transfusion is indicated in cases chiefly of loss of blood and in certain septic conditions in which hemolysis has gone on to a dangerous extent. At any rate, we are in a dilemma as to what to do in regard to the use of drugs. I have used epinephrin occasionally for a short time only and watching blood-pressure. We do know that in severe shock we have a lowered blood-pressure, and anything which will help to bridge over the sudden depression is of some use.

Dr. John F. Binnie (Kansas City, Mo.): There is only one point which I desire to mention in connection with shock, even although it is so well known; and that is the application of heat. Have we not in the application of heat an explanation of the good results we so often get from filling up the patient with salt solution?

Dr. Corbett (closing): I wish to thank the members of the Association for the discussion they have given my paper. In reply I wish to say, we have a symptom syndrome that we call "shock." This syndrome may result from infection, hemorrhage, trauma, exhaustion, or fright, or any combination of the above elements. My paper deals with a very narrow field or part of shock, that is, the kind of shock we see in the experimental laboratory when we traumatize the peritoneal cavity of an animal, and the kind of shock we sometimes see on the operating-table. The immediate etiological factors producing this syndrome are trauma and anesthesia.

Some one said that a lowered blood-pressure is a conservative reaction, and that it occurs for the protection of the

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patient, and that it is something which ought not to be recklessly interfered with. But in these cases of extreme shock we have a patient, not only whose blood-pressure is failing, but in whom failure of respiration seems imminent. Something must be done to prevent death. In shock failure of respiration occurs ordinarily before cessation of the heart's action; therefore, I think in shock failure of respiration is oftentimes the cause of death.

When we raise the blood-pressure by a transfusion, the respiratory function seems to be restored. In explanation of this I have assumed that in a normal individual the intracranial pressure and the blood-pressure are about equal, and there are certain anatomical conditions that tend to maintain this equilibrium. However, with a sudden fall in blood-pressure the intracranial pressure may for a time exceed the general blood-pressure. When the intracranial pressure becomes greater than the blood-pressure, respiration ceases, presumably because the blood is forced out of the respiratory centers. I have attempted to illustrate this by an experiment. By placing a cannula in the skull of a dog, another cannula in the spinal canal, and a final cannula in the carotid, I obtained intracranial, intraspinal, and blood-pressure tracings. Whenever the intracranial pressure was made to exceed the blood-pressure then respiration failed. Asphyxial rise of blood-pressure naturally for a time attempted to raise the blood-pressure, but never was normal respiration re-established until the intracranial pressure was brought down to the blood-pressure. Therefore, in those cases of shock where death is imminent, or impending from respiratory failure, we must either lower the intracranial pressure or bring up the blood-pressure. I have seen clinical favorable results from lowering the intracranial pressure, and I have also seen clinical favorable results from raising the blood-pressure.

The point raised by Dr. Seelig is an excellent one, that is, following the administration of epinephrin we ordinarily get a prompt rise of blood-pressure followed by a secondary fall. This is the rule following super-physiological doses of epinephrin, but in these shocked animals where we used only physiological doses,—that is, just enough epinephrin to maintain blood-vessel tone,—we got a primary rise and slight secondary fall, but the blood-pressure after the secondary fall was at a higher level than it had been before the administration of

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the epinephrin. This only holds true with the continuous administration of physiological doses.

Dr. Grant has asked why more blood should be put into the circulation in condition of shock. In answer to that I want to say that in this variety of shock we are practically dealing with hemorrhage. The hemorrhage, though not visible because of the escape into the tissues, is none the less hemorrhage. As we put these animals through shock, fluid was collected from the peritoneal cavity, and large amounts of low-specific-gravity fluid were found to accumulate there. At the same time the blood itself showed increase in specific gravity. Therefore, there apparently was concentration of the blood in the blood-vessels and loss of low-specific-gravity fluid into the tissues. Further than this, animal experiments and clinical experience in the series that I reported showed beneficent effect from the administration of epinephrin when supplemented with transfusion. However, I wish to reiterate that epinephrin is a dangerous remedy unless controlled by blood-pressure findings.

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THROMBO-ANGEITIS OBLITERANS OF BUERGER—INTERMITTENT CLAUDICATION

(SPONTANEOUS GANGRENE IN THE YOUNG)

LEWIS L. McARTHUR, M. D., F. A. C. S.

CHICAGO, ILLINOIS

Spontaneous gangrene of the extremities, usually in the feet, but occasionally in the hands, until recent years has been accepted as an ailment which, if not associated with diabetes, was to be regarded as a sequel to an irremediable vascular disease, for which amputation sooner or later must be made.

With refinements in diagnosis and laboratory technic we are now compelled to recognize the existence of at least five distinct types:

1. Diabetic gangrene.
2. Endarteritis obliterans, with the arteriosclerosis of the aged. (Pott's sore toe.)
3. Syphilitic endarteritis.
4. Reynaud's disease.
5. The spontaneous gangrene of the young (18 to 48), the thrombo-angeitis of Buerger (intermittent claudication).

I desire to invite your attention to the last-named type in a brief communication, not because I have anything original to offer, but to add to your armamentarium a method of relief, as yet not sufficiently known, for these pitiable sufferers.

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Given, then, a case of impending or existing gangrene in a relatively young individual, in whom no evidences exist of a diabetes; who neither gives a history of syphilis, nor yields a positive Wassermann; whose arterial tension is not abnormally high; who neither presents the signs of a general arteriosclerosis nor the vasomotor characteristics of a true Reynaud's disease —he or she must be, for the present, classified as of the thrombo-angeitis type. Since one of the very early manifestations (more frequently met with in this type than in other forms of gangrene, but not absolutely confined to it) is a limp or lameness, the term "intermittent claudication," really only a symptom, has also been used to designate it. This limp is caused by a cramp occurring in the muscles of the calf of the leg, coming on suddenly, usually after a short walk, quite painful in character, but not of long duration, only to recur on repetition of the walk. Associated with this are a sluggish circulatory change in the foot, giving it a reddish or bluish appearance, and cold feet. On palpation the pulse in the dorsalis pedis and posterior tibialis may be feeble or wholly missing. That there is a retardation or sluggishness to the flow has been both emphasized and measured by Moszcovitz. He compared the average time in which, after a five-minute constriction of the vessels of the ankle in a normal individual, the active red hyperemia recurred (three to five seconds), with the delayed return in this disease (sometimes up to sixty or more seconds), using this test as a diagnostic confirmation of retardation. Overshadowing every other manifestation, however, is pain of a more or less persistent type and often of most atrocious intensity. This pain is increased by lowering the foot and by changes in its temperature. It is unusually intractable to morphia and the anodynes. It is regarded by most writers as a nerve

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protest to the poor circulation. Because of the striking relief from this pain by a simple improvement of the circulation, I have felt justified in recalling to your attention a procedure that has been buried in the Archives of the Japanese Surgical Congress since 1911. At that meeting G. Koga presented his amazing results in thirteen cases by his treatment, with eleven cures and two recurrences. His colleague, J. Mayesima, had noted that the viscosity of the blood in certain cases of gangrene in the young was distinctly and constantly higher than normal. Through Mayesima's most elaborate studies of the viscosity of the blood, Koga concluded that by reducing this he could improve the circulation in this rather common affection in Japan. It has long been known that the viscosity of the blood can be modified in various ways, the simplest of which is its dilution by Ringer's or Locke's solution:

Locke's:

$NaHCo_3$	0.015 per cent
$CaCl_2$	0.024 per cent
KCl	0.042 per cent
NaCl	0.092 per cent
Glucose	0.100 per cent

Ringer's:

$NaHCo_3$	0.010 per cent
$CaCl_2$	0.024 per cent
KCl	0.042 per cent
NaCl	0.900 per cent

A brief digression might here be made long enough to quote Mayesima freely, that practically all the methods of determination of the viscosity of the blood, except that of Robert Tissot, are dependent upon its rate of flow through glass capillary tubes after Poisseuille's law. Mayesima utilized the Hess viscometer, which

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compares the flow of the liquids through capillary tubes, being tested with the flow of water, taken as one. He has covered exhaustively the entire field and literature of viscosity of blood, in an admirable monograph (Mitt. a. d. Grenzgeb. Med. u. Chir., 1912, Bd. 24). The average viscosity for both sexes, 4.16; for men, 4.4; for women, 3.76. The normal average for men, 3.8 to 4.8; for women, 3.4 to 4.1. The viscosity of the blood of Japanese is lower than that of Europeans, and in any individual varies with the seasons, the time of day, the nature and amount of food ingested, the excessive use of baths, the age, and the sex. These variations, however, are not so great as to negate the value of knowing the viscosity in many pathological processes. The viscosity is dependent upon both the physical and chemical composition of the blood, so therefore the greater the number of red or white corpuscles, the higher the viscosity; the more extensive the operation—for example, amputation of the mammæ or hysterectomy for fibroid—the greater the reduction. But peritoneal and bone-marrow irritation raise it greatly. An excessive meat diet raises the viscosity; a vegetable diet lowers it. Certain medicaments, for example, KI, lower it slightly. *Dilution of the blood lowers it.* The coagulability time is inversely proportional to the viscosity. Fluids with diminishing viscosity flow through capillaries with increasing rapidity, hence the frequent dilution of the patient's blood increases the rapidity of flow through the narrowed lumina of those diseased blood-vessels producing the gangrene.

Utilizing this knowledge and the observed fact that all cases of spontaneous gangrene had an abnormally high viscosity, Koga, by injecting at first daily, later on alternate days, 1,000 c.c. of physiologic solution into the subcutaneous tissues of his gangrene cases, ob-

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tained the results above quoted. Dr. Wylly Meyer, of New York, has had, as have I, abundant opportunity to test out, with gratifying results, this treatment, because of our connection with Jewish hospitals. In this country seven out of eight cases are found among the Jewish race, and a similar frequency in the men as compared with women. Almost all are inveterate smokers, and highly neurotic. Quoting from Koga:

"Hand in hand with the lowering of the blood viscosity, the symptoms frankly improve, the edematous swelling diminishes, the cyanosis shows less and less. A sharp line of demarkation forms, ulcers clean up and cicatrize, and the peripheral blood-vessels, which prior to the infusions were not palpable, now pulsate. Subjectively, the pains grow less and less, to finally vanish, so that the patients can rest at night undisturbed. They complain no longer of a sense of cold, and finally can make longer and longer walks without intermittent claudication appearing."

So startling sometimes is the relief of pain, so unusual to see in these sluggish circulations the color of the part change to normal; so sharp the line of demarkation between living and dead tissue as to excite surprise; and the toes and feet, formerly removed by very high amputation, are now often seen to rid themselves of their dead tissues, granulate, and cicatrize. Patients confined to bed, confirmed drug-takers, and constant sufferers have been restored to health without the loss of their feet and have been able to resume their activities. Most striking of all is it to feel the pulse return in a radial artery, the absence of which has been commented on by the nurse, whose duty it was to record same, in a patient afflicted in both feet and hands; or, after the absence of the dorsalis pedis and the posterior tibial pulse for weeks, to find it return with the improvement and cicatrization of the

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gangrenous areas. In an examination of the vessels of a leg, amputated for this ailment three years ago, the popliteal artery was found normal to the upper third of the calf, where an organized thrombus was located, obliterating the vessels for the distance of two or more inches. Below this area a vessel was again found whose walls and lumen were normal in every particular. This finding corresponds to that of Leo Bürger, who has noted even the canalization of such an organized thrombus with small new channels. To his studies of the vessel-changes in this affection is due the present identification as a separate entity of this variety of gangrene, hence "thrombo-angeitis—Bürger." Yet the active surgeon will resent somewhat the introduction of this treatment, because it requires a varying number of injections (from 5 to 30) of 1,000 c.c. of a solution that must be prepared with a perfect aseptic technic, and be given by one competent to do a painless and safe hypodermoclysis. However, at most hospitals, after the technic has once been demonstrated, the resident interne can carry out the treatment. It has this objection, that it takes at least an hour each time, and must extend over a period of six to eight weeks in the average case before it can be discontinued. In the cases reported by Koga (Deutsche Zeitschrift für Chirurgie, 1913, 121) they remained well from five months to two years, the viscosity of the blood continuing below that at which they began treatment. This coincides with the results obtained in my service in the cases of eight patients in whom treatment was carried out, though my experience with it has extended only over a period of about eighteen months. The results (20 per cent cures; 60 per cent improved; 20 per cent unimproved) have been sufficiently satisfying to the writer to convince him his duty to his patient demands at least its trial prior to

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resorting to high amputation, and so convincing to the patient that for the past six months there have been from one to five cases in the wards, most of the time, sent there by those previously benefited. While not every case may be cured by this procedure so many have now been relieved in my service, that I believe the Fellows will not regret giving it a trial.

DISCUSSION

DR. A. C. STOKES (Omaha, Neb.): I would like to ask Dr. McArthur what his treatment was. I understood him to say it was 1,000 c.c. of Ringer's solution. Is that correct?

DR. MCARTHUR: Yes.

DR. STOKES: I have had some experience with the class of cases related by Dr. McArthur because I happen to be connected with a Jewish hospital. We have had in the last three or four years four cases of the condition he has mentioned. In the first one we attempted to anastomose a large vein with the femoral artery, but this failed, and we had to amputate. Then we accidentally saw an account of the treatment the doctor has described, and we tried it in the other cases and got better results. We tried it in three cases, in one of which we had finally to amputate.

Professor von Bergmann made a statement many years ago which impressed me deeply, namely, that these cases always occurred in inveterate cigarette-smokers, and they are almost always Russians. The cases that have come under our observation have been Russian Jews, who are inveterate cigarette-smokers.

DR. MCARTHUR (closing): In looking over the literature in connection with viscosity I have found some interesting facts, one of which is, that one may make a differential diagnosis between fracture of a large bone and dislocation. In fractures the viscosity of the blood is high, but it is not changed with dislocation.

In peritonitis there is always a very marked rise in viscosity; with improvement in the peritonitis there is a corresponding drop. In bone operations, *particularly those affecting* the *bone marrow,* a high viscosity is shown, so that it becomes relatively clear. For instance, in peritonitis there is a very marked in-

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creased leucocytosis, and as the physical composition of the blood determines in part its viscosity, the increased number of leucocytes retards its flow and raises viscosity. With improvement in peritonitis the viscosity drops.

I would like to see some of you brave enough to try this method also in diabetes and in cases of senile gangrene. I have hesitated making prolonged injections in a diabetic patient. There is no record in regard to viscosity in these cases, but I believe their blood viscosity should be very high on account of the sugar content, and by lowering their viscosity in impending gangrene we may improve their condition. Of course free use of fluids both as drinks and rectal instillations are in the right direction toward a reduced viscosity.

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DISLOCATIONS OF THE KNEE

JOSEPH RANSOHOFF, M.D., F.R.C.S. (Eng.), F A.C.S.

CINCINNATI, OHIO

On the authority of the late John Shaw Billings,[1] dislocations were far more numerous in the time of Hippocrates than they are now. This was probably true also of dislocations at the knee. Notwithstanding the great increase of industrial accidents, the latter are still very uncommon. Until Malgaigne first classified them from a collection of eighty cases, in 1855, there was, to my knowledge, no classification of injuries about the knee other than fractures.

In 1894, Cramer[2] had added one hundred and ninety other cases. In 1907 Otto[3] had added fifty-five further cases, and Magda Frei[4] in the following six years was enabled to collect twelve.

The broad apposed surfaces of the knee are held firmly together by a strong capsule and lateral and crucial ligaments, which must always make dislocations here uncommon. Furthermore, the motion of the knee, limited absolutely to one plane, except in the final locking of the joint for support and in the slight rotation possible during flexion adds to the knee's stability.

In the records of the Cincinnati Hospital for the last twenty-eight years there have been only three dislocations of the knee. Curiously enough, two of these occurred in 1905, in the service of the late Doctor Dandridge.

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Among one thousand dislocations in the hospital at Copenhagen, there were only two of the knee. The strong popliteal tendons and the strong capsular ligament with the quadriceps tendon and patella so add to the stability of the joint that only a tremendous force can pry the surfaces from each other, and that only with extensive derangement within the knee, and much tearing of the ensheathing capsule.

The ease with which dislocations of this joint are reduced makes it probable that some of them escape observation in hospitals, because they are reduced on the outside by simple traction, applied by some lay exponent of first aid to the injured.

The following cases occurred in the Cincinnati Hospital:

CASE 1.—P. K., aged 34, admitted to service of Dr. Dandridge, June 7, 1905. A packing clerk. Fell from second story window while intoxicated. Does not know how he landed. The right knee presents a large hematoma on the inner aspect. Preternatural mobility. Leg dislocated backwards and outwards. Patella dislocated to the outer side, and on edge. Blood supply of leg and foot apparently intact. Pulsation in posterior tibial and dorsalis pedis normal. Reduction easily accomplished with patient discharged as well four weeks after admission.

CASE 2.—Service of Drs. Dandridge and Oliver. W. C., aged 35, male, admitted Nov. 29, 1905. Laborer. Was loading boxes in garbage-wagon when he slipped off the wheel, and his right leg slipped between the bars of grating covering the cellar. The body was pitched to the right side, and his leg dislocated. The right knee shows bruises over external tuberosity with knee dislocated outwards, although the dislocation is not a complete one. Extension and flexion are possible. An *x*-ray was not taken. Reduction easily accomplished. Treated by posterior splints, and discharged improved on the twenty-third day. The degree of functional impairment was not given in the history.

CASE 3.—Service of Dr. Ransohoff. F. B., aged 33, male. Ironworker. Family and previous history, unimportant, ex-

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cept for a luetic infection, which was treated with salvarsan. While doing some riveting work on a truss he slipped and fell about forty-eight feet, and lighted on a pile of junk. When questioned as to how he struck, he did not know. This is hardly remarkable. He also sustained a fracture of the lower jaw.

The right leg for a distance of about eight inches above the knee is greatly swollen, and contains a large hematoma. On the anterior surface of the leg below the knee there is a superficial contused wound less than an inch in each diameter. The right leg below the knee is of a purplish color, and is slightly swollen. The region of the knee presents a marked deformity. The long axis of the leg and thigh is broken at the knee, with an evident outward and forward displacement of the leg, presenting a typical bayonet appearance. The leg is in hyperextension, and rotated nearly ninety degrees at the knee. The patella is drawn above the condyles and movable on the antero-external surface of the femur. The quadriceps tendon is greatly relaxed. The upper surface of the tibia can readily be outlined in part, and the rounded ends of the femoral condyles can be felt, projecting into the popliteal space, almost beneath the skin. Lateral movements are easily made, and the preternatural mobility in almost every direction reminds one of the description of similar movements in a case of Kirmisson[5], who aptly compares the range of movements to that of a jumping jack (punchinello).

The shortening of the limb is about one and one-half inches. The arterial pulse can be felt in both posterior tibial and dorsalis pedis. There is numbness in the leg and foot, but the tactile sense is not impaired.

The *x*-ray (Figs. 1 and 2) shows a complete forward and outward dislocation of the leg. The tubercles of the spinous process of the tibia show a slight roughness along their edges, but no evidence of their having been torn loose. Although the interpretation of the plate is somewhat difficult, it seems certain that there has been no fracture.

The reduction was easily accomplished under anesthesia. Indeed, it was so easily accomplished that an *x*-ray picture taken the following day showed that a slight internal dislocation had been produced.

This was easily remedied. A posterior splint was applied, and the patient left the hospital with the functions of the knee nearly normal on the twenty-fifth day. He has been

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under the care of the Gymnastic Department of the Cincinnati Hospital, where by baking and massage a practically normal knee-function has been obtained.

In a little over 40 per cent of all cases of dislocations of the knee the leg is dislocated forward, in about 20 per cent backward, and 20 per cent outward, and inward in about 7 per cent. Such, at least, are the conclusions drawn from the latest German tabulations.

A study of all such statistics on knee-dislocations shows the evident desire to classify every case categorically, according to the relations of the dislocated leg to one of the cardinal points of the knee's circumference.

As a matter of fact many of the dislocations forward and backward are complicated by a partial or complete lateral displacement, preferably to the outer side. This was true in our case.

As the parts of the hinge can be displaced only in the axis of its motion, unless there is an extensive break in its interlocking parts, I have been convinced, from a study of cases reported and from some little experimental work on the cadaver, that practically all dislocations of the knee, unless attended by extensive fracture, are primarily either forward or backward. The lateral displacements and the rotation of the leg to ninety degrees, which is occasionally encountered, I believe to result from the continuation of the force on the leg already dislocated. It is highly probable that violent muscular action has a large part in determining the position the dislocated leg finally assumes. The high position of the patella, often on edge, along the outer side of the knee, or as in the case of its complete inversion recently reported by Frei, speaks forcibly for the effect of the quadriceps contraction after the primary dislocation has taken place.

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Dislocation of the knee by rotation alone of the leg, or of a rotation plus abduction, can doubtless occur, but it is rare. Cramer has been enabled to collect nine such cases.

The most interesting case of rotation dislocation was reported by Frei, and it was complicated by complete inversion of the patella. The injury was a compound one, so that the drawing made by the author is accurate. It is of sufficient interest to warrant its reproduction here. (Fig. 3.) The injury was the result of an automobile accident, and, curiously enough, in two other recent reported cases by Hering[6] the dislocations were similarly produced.

The mechanics of dislocation at the knee have been studied sufficiently, both in the living and the dead, to make it certain that anterior dislocations almost always occur while the limb is in extreme extension, and the posterior dislocation while it is in flexion.

Since it is the function of the anterior crucial ligament to prevent the tibia being displaced forward, it is invariably torn, or the tibial spines give way before a dislocation can take place.

The most interesting and unique series of cases of anterior dislocation was reported by Eames,[7] in 1890. The accident resulted from the too rapid fall of the elevator in a mine shaft. Five out of eighteen occupants sustained an anterior dislocation of the knee. The estimated fall of the cage was sixty yards. The position of the man while in the cage is supposed to be with extended knees, the upper part of the body leaning forward.

In two of these cases the skin was split across the back of the knee. All of his cases recovered, and within a relatively short time were again at work, on full time, in the mines.

As a result of a similar accident, reported by Her-

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ing, in which six miners were injured by the falling of a cage in a mine-shaft, two sustained a forward dislocation of the knee. A third forward dislocation was observed by him from a similar accident three weeks later. In two of the cases, amputation was made necessary on account of gangrene following injury to the popliteal vessels.

It would appear therefore that forward dislocations of the knee would be almost typical of the too rapid descent of the cab in mine-shafts.

Parenthetically, it may be stated that complete recovery of joint function in these cases is in marked contrast with the slow recovery which attends seemingly less severe injuries of the knee, such as rupture of the crucial ligaments or the avulsion of the spinous process of the tibia. The contrast is all the stronger because it is impossible to conceive an anterior or posterior dislocation of the knee without the tearing off of one or both of these ligaments. That hyperextension may occasionally produce a backward dislocation is shown by the case of Stetter.[8] A soldier, running rapidly down the hill, suddenly straightened up to salute a passing officer, and at once collapsed with a posterior dislocation of the knee.

In a very few cases a great weight falling on the lower part of the thigh, while the knee was flexed, produced a forward dislocation.

Backward dislocations are, as a rule, the result of force applied to the leg while the limb is in flexion. It cannot occur without rupture of the crucial ligaments. According to Hardoin,[9] autopsy, amputation, or operation has made a true study of lesions possible in twenty-seven cases. In thirteen there was direct backward dislocation. In three there was backward and outward, and in six the dislocations were accompanied by an outward rotation and in one by rotation inwards.

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In eleven out of twelve complete backward dislocations, the crucial ligaments were torn. Except for the avulsion of the spinous processes fractures are very rare as an accompaniment of dislocation of the knee.

Fowler,[10] however, has recently described a forward dislocation with tearing of the anterior crucial ligament and an extensive fracture of the head of the tibia extending into the joint. The patient died of pneumonia ten days after the accident, thus permitting a careful autopsy of the joint.

The most serious danger of dislocations in this region is from damage of the blood-vessels in the popliteal space. In the third case recorded, although there was great discoloration of the knee from venous stasis, which rapidly disappeared after reduction, there was no interference with arterial circulation. In very many instances the artery or the vein, or both, are torn, necessitating either primary sutures or amputation. In a number of instances the former has been attempted, but, so far as I have been enabled to glean, without success. The ends of the divided artery have either been so far separated from each other as to preclude a suture, or there has been concomitant injury to the vein, with the practical certainty of gangrene, which made amputation necessary.

Although it would seem that in posterior dislocations the vessels would be more likely impinged upon by the upper sharp edge of the tibia, and therefore lacerated more frequently than in anterior dislocations, the reverse is true. The over-stretching consequent on the anterior dislocations is more disastrous to both artery and vein. The gravity of vascular complications becomes evident from the statistics of Hardoin, according to whom twelve out of twenty-seven cases were of posterior dislocations, with lesions of the

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popliteal vessels, which necessitated amputation, when death did not occur before operation.

Wissner[11] collected twenty-one cases of anterior dislocation with rupture of vessels. While theoretically it would seem probable that rupture of the main artery might be followed by an adequate collateral circulation, the contusion and laceration of the periarticular soft parts is such that more or less extensive gangrene has invariably followed. The greater resistance of the popliteal nerves comes in good stead in knee-dislocation, very few cases being recorded in which permanent functional loss was observed.

In these dislocations, the laceration of the capsule and ligaments is usually so extensive that with little traction and manipulation reduction is readily accomplished. Nevertheless, fifteen cases have been recorded in which an open operation was done before reduction could be accomplished. Ruppauer[12] collected twelve cases in 1906. Three cases have since been recorded by Iselin, Frei, and Mauclaire.[13]

A narrow tear in the capsule or an interposed semilunar cartilage has most often caused the irreducibility.

Frei collected twenty-five cases of compound dislocation, nine of which ended fatally. In five, amputation was successfully performed. Of nine, reduction could be effected with good functional result in seven.

BIBLIOGRAPHY

1. Billings: Dennis Syst. Surg., vol. 1, p. 23.
2. Cramer: Dissert., Wurzburg, 1894.
3. Otto: Dissert., Berlin, 1907.
4. Frei: Deutsch. Ztschr. f. Chir., vol. 125, p. 175.
5. Kirmisson: Bull. Soc. de Chir. 1913, p. 1039.
6. Hering: Beitr. klin. Chir., vol. 83, p. 352.
7. Eames: Brit. Med. Jour., 1900, vol. 1, p. 958.
8. Stetter: Quoted by Ruppauer.
9. Hardoin: Rev. de Chir., 1914, pp. 327 and 478.
10. Fowler: Jour. Am. Med. Assn., 1911, vol. 57, p. 2124.
11. Wissner: Dissert., Leipzig, 1910.
12. Ruppauer: Deutsch. Ztsch., vol. 83, p. 554.
13. Mauclaire: Soc. de chir., 1913, Bull. No. 131.

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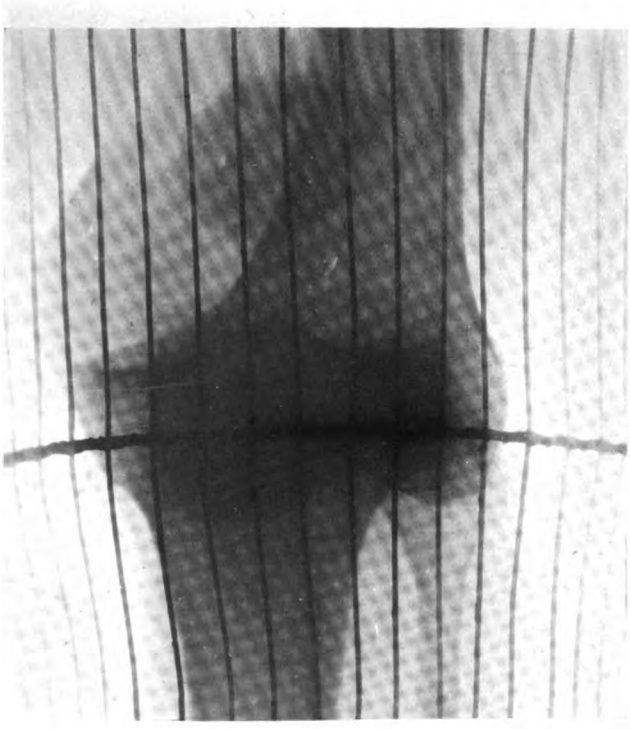

Fig. 1.

Fig. 2.

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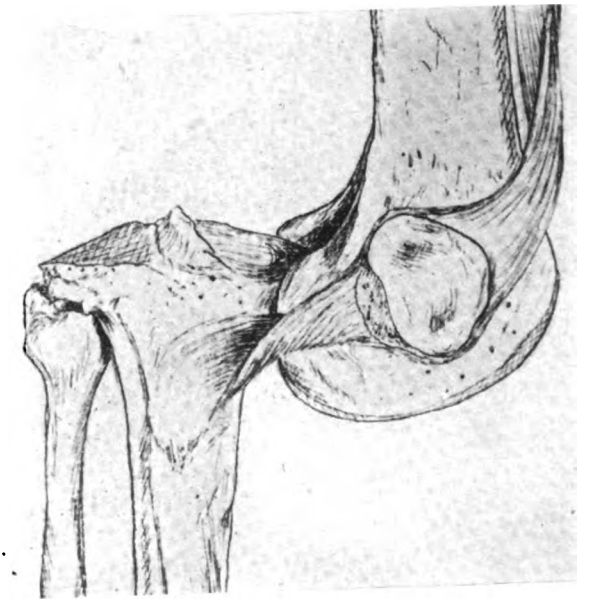

Fig. 3.

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DISCUSSION

DR. EARLE R. HARE (Minneapolis, Minn.): I rise to express my great interest in the paper which has just been read and to report a single case of partial posterior dislocation of the knee-joint with rupture of the anterior crucial ligament due to a severe traumatism upon the leg. I do not believe it is possible to have a dislocation of the knee-joint in any direction without laceration of the crucial ligaments. The anterior crucial ligament prevents forward dislocation of the leg; the posterior crucial ligament prevents posterior dislocation. These cases are more common than we are led to believe; for, if I understand the essayist correctly, he said only three hundred cases have been reported. I believe they are much more common than that, because a large majority of the cases are not reported. I am inclined to think that it is a rather common injury of the knee-joint.

DR. ALBERT E. HALSTEAD (Chicago, Ill.): I have seen two cases of backward dislocation of the tibia at the knee-joint. These are properly termed posterior dislocations of the knee. Both of these were in horse-shoers and were the result of an accident, the horse falling on a man's leg while the knee was flexed. The leg was relaxed, and the knee partially flexed with the horse's leg over the man's knee; the horse falling dislocated the knee backward. Two such accidents have been treated by me. One occurred about twenty years ago, and the other four years ago. The first one was seen in private practice, and the second one in St. Luke's Hospital.

Last fall I saw a man who had had a severe injury from an automobile accident. The car, he claimed, passed over both of his legs. When I saw him he had an anterior dislocation of one knee (tibia dislocated forward), with gangrene of the leg from rupture of the popliteal vessels. The doctor in charge said he had a dislocation of both knees, and that he had reduced the other dislocation, but had failed to reduce the one in which gangrene had occurred. On account of swelling he did not recognize the nature of the injury. I though that the other knee had been dislocated because of the great relaxation and free mobility of the joint. One could put the knee in almost complete dislocation without any difficulty.

These injuries are not so rare as Dr. Ransohoff would have us believe. Personally, I have known of several other cases that occurred in the practice of my colleagues in the hospital

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during the last twenty years, and I saw these three cases in my own practice.

DR. RANSOHOFF (closing): These accidents are exceedingly common, and I stated in my paper that many of them did not come under the observation of the surgeon at all. There are people who become so accustomed to first aid that their dislocations are reduced very often on the outside, and they do not reach the surgeon; but an anterior dislocation of the knee in a hospital is a rather uncommon thing.

The automobile, without question, is an important factor in increasing the number of dislocations of the knee. In looking over the recent literature of the subject, I found many of them were the result of automobile accidents.

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FOREIGN BODIES IN THE URINARY BLADDER

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Foreign bodies usually enter the urinary bladder through the urethra. In many of the reported cases, a piece of a surgical instrument, for example, rubber catheter or dilating instrument of some kind, has been broken off or lost during the process of examination or treatment.

Packard[1] has collected and analyzed 222 cases, including one of his own, of foreign bodies introduced into the male bladder through the urethra. In 108 instances the object was a part of a surgical instrument. In the remaining 113 there was included a wide variety of articles, almost all different. The foreign body was usually introduced into the urethra purposely, and then lost control of, when it slipped back through the urethra into the bladder. The length of time the objects had remained in the bladder, varied greatly; in many instances it is difficult to determine just how long they have stayed in the urethra before entering the bladder. In Packard's own case, a piece of hard-rubber bar had been in the bladder fourteen years. Other authentic cases have been reported of foreign bodies in the bladder for at least fifteen years, and when removed they were usually entirely surrounded by incrustations from salt deposits. Some materials, how-

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ever, seem to be immune to this; in one of our cases, a piece of bone had been in the bladder for some time, and showed very little incrustation.

In several instances in our Clinic pieces of catheters have been removed, the histories showing that they had probably been in the bladder for several weeks. They were usually discovered during the examination of patients for enlarged prostates or for stones in the bladder, and have been removed when operating for such conditions. The following are brief histories of cases of foreign bodies in the bladder which were treated in our Clinic:

CASE 1 (A19437).—D. K., female, aged 7 years. Examined January 25, 1909. The patient thought a hairpin got into her bladder about eleven months before. Since then she has had burning on micturition (better and worse by spells) and frequency, with the passage of small amounts of urine at a time; never any hematuria. Bowels, normal; appetite, good; no cramps. Urinalysis showed alkaline reaction, and some pus and albumin. Röntgenogram taken elsewhere showed the hairpin and a shadow in the area of the bladder. A cystoscopic examination was made, and a wire hairpin imbedded in the left wall of the bladder close to the urethra was discovered. The free end of the hairpin was encrusted with a calcareous deposit. The inserted end was S-shaped, and could not be removed through a Kelly cystoscope. The child was operated on January 27, 1909; and the hairpin, covered with stony incrustation, was removed through the urethra.

CASE 2 (A73442).—A. R., female, aged 13 years. This patient was examined September 10, 1912. She had been troubled with enuresis since the age of 9. After exercise there was present a dull ache, referred to both loins, particularly to the right. For the past nine months there had been considerable irritability of the bladder, and incontinence most of the time; no obstruction, but a great deal of urgency. Stone in the bladder was palpable per rectum. Urinalysis showed acid reaction, a trace of albumin, a few red cells, and a good deal of pus. In the röntgenogram the shadow of a hairpin, with what appeared to be a phosphatic deposit about it, showed in the vesical area. Examination with the cysto-

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scope revealed a markedly inflamed bladder, and a large, rough phosphatic stone the size of an egg. The pin was not visible; however, it was impossible to thoroughly explore the bladder. Suprapubic cystotomy was done September 12, 1912. A large stone, which formed about the pin as a nucleus, was removed, and a retention catheter inserted into the urethra (Fig. 1).

Case 3 (A112539).—N. C., female, aged 17 years. Examined August 10, 1914. Nothing of note in previous history, except a gradual decrease in the menstrual flow for the past five months, there being at the last period only a slight stain. For one year there had been urinary frequency, which was gradually getting worse. Ten months before there had been acute pain, not radiating, and lasting for several days, in the region of the right kidney. The patient was in bed three or four days. Pus, but no blood, was noted in the urine a day or two afterward. She had had four similar attacks since then, though during the last one the pain was not as severe and of shorter duration. There had been increasing frequency and burning with a decreasing amount of urine, and occasional incontinence at night and sometimes during the day. The urine, starting and stopping by spells, was then induced to flow again by straining. Bowels, constipated. Appetite, fair.

She said she had used a hairpin about a year ago because she was unable to pass water. The physical examination showed hypertrophic breasts, pigmentation, and abdominal striæ, suggestive of previous pregnancy, which the girl denied. The vagina admitted two fingers. The blood on the examining fingers was probably menstrual. The uterus was small and retroflex. Urinalysis was negative. Röntgen and cystoscopic examinations revealed a small, irritable bladder; marked cystitis; one stone the size of a hen's egg, irregularly elongated, rough, phosphatic, light-gray in color. A diagnosis was made of stone in the bladder, probably with a hairpin as a nucleus. Suprapubic cystotomy was performed under ether anesthesia on August 14, 1914. The stone was removed, and a tube drain inserted outside the bladder (Fig. 2).

Case 4 (A95006).—R. F., male, aged 10 years. Examined November 5, 1913. Six hours before being brought to the Clinic, November 3, 1913, this patient was shot in the abdomen with a 22-rifle, the bullet striking midway between the umbili-

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cus and the pubes. He had vomited twice, and was passing bloody urine; pulse, 90. It was believed that the bladder and probably the intestine had been perforated. He was operated on at once and the bullet which had entered the bladder through the peritoneal surface was removed. The opening was sutured with catgut, and drainage inserted. The bullet had penetrated one side of a loop of the small intestine, and an ecchymotic spot was plainly visible on the opposite wall. This opening was also sutured.

CASE 5 (Gr. Book 5, p. 432).—G. W., male, aged 20 years. Examined October 31, 1902. For the past two months there had been frequency of urination, and pain and sometimes blood at the end of micturition. The patient admitted having inserted gum into the urethra. Pus and blood were found in the urine in considerable quantities. A diagnosis was made of stone or foreign body in the bladder. November 8, 1902, a suprapubic cystotomy was done, and a large piece of chewing-gum with stony incrustation removed.

I wish particularly to call attention to two cases that are unique, at least in some respects. In both instances the foreign body had evidently passed into the bladder through the side wall. It is of interest to note that this occurred and produced so few symptoms.

CASE 6 (A109459).—J. M., male, aged 21 years. This patient came for examination July 1, 1914. Twelve years before, an open jack-knife had been thrown at him, striking his left hip just posterior to the great trochanter. A large blade of the knife was broken off, and was never found. After the injury he was confined to his bed for three weeks. No trouble since, except slight frequency by day for a long time. Twenty-one months before the examination, he noted slightly increased frequency, and after two weeks for two days the urine contained a few drops of blood at the end of micturition. The winter following, he passed urine about once at night and every hour during the day. In the summer of 1913, he noted smarting in the urethra after urination. One year ago, after riding horseback, all of his urine was bloody for a few days. On several occasions he had noted that severe jolting caused pain in the genitalia, followed by hematuria. For several months he had had constant hema-

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turia and pain in the urethra and perineum, the urine flowing freely at times, at others, starting, stopping, and dribbling with marked urgency. Three weeks before coming to the Clinic, he had had severe pain for an hour in the region of the left hypochondrium, radiating to the back. This was accompanied by nausea. He had coughed for at least a year, sometimes expectorating mucus streaked with blood. Occasionally he had night sweats. Weight, normal. On physical examination there was tenderness per rectum, with an indefinite mass at the left, anteriorly, above the prostate. A scar on the left hip one inch long, vertical and posterior to the great trochanter, was noted. Specific gravity of urine, 1019; some albumin; a few red-blood cells; and a considerable amount of pus. Examinations negative for tubercle bacilli in the sputum and urine. Röntgen and cystoscopic examinations showed a large stone in the bladder formed about a foreign body, and a moderate degree of cystitis. On July 10, 1914, a cystotomy was done, and a large stone formed on a knife-blade removed. The interior wall of the bladder was carefully examined, but neither wound nor scar could be found to show where the knife blade had entered (Fig. 3).

Case 7 (A71701).—T. B., male, aged 18 years. This patient was examined August 6, 1912. In 1909 he had been operated on elsewhere for the removal of a sequestrum in the region of the left os calcis. He was up and around at the end of two weeks. At that time he had a great deal of pain and tenderness in the region of the left femur. Disturbance of the bladder was first noted in February or March, 1909; pain in the bladder occurred just previous to this. He stated that some time in March, 1909, he had passed a piece of bone through the urethra, and that during 1910, 1911, and 1912 there was very often blood, sometimes clots, in his urine. In 1913 he again passed fragments of bone with the urine. There was marked atrophy of the left leg with shortening and fixation, old sinuses of the left hip and heel, swelling in the region of the right hip, small abscess on the right forearm, and palpable cervical and inguinal glands. During 1912, 1913, and 1914 he visited the Clinic several times to have plaster-of-Paris cases applied for tuberculous osteomyelitis of the left femur. When he came for consultation, the infection had already extended into the hip-joint. Röntgen and cysto-

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scopic examinations showed the shadow of a detached piece of bone in the bladder. Tubercle bacilli could not be demonstrated in either the urine or the sputum.

He was operated on September 23, 1914 (suprapubic cystotomy), and the piece of bone removed. Examination of the interior lining of the bladder did not reveal any scars, nor was there any clue as to how this fragment of bone, evidently a piece of sequestrum, got into the bladder. Doubtless it came from the femur, the result of the old tuberculous osteomyelitis. February 8, 1915, the patient wrote that he was feeling well, and had gained twelve pounds. He notices that when he works hard there is a filamentous and string-like substance in the urine; at other times it is clear. All of the patients made uneventful recoveries. (Fig. 4.)

A number of instances have been reported (Freeman[2], Harrison[3], Roberts[4]), in which objects have been removed from the urinary bladder that presumably had been swallowed. A case somewhat similar to those I have reported, is published by Ghose,[5] who performed a lateral cystotomy on a boy five years old, and removed from his bladder a splinter of wood, which had been driven into his perineum by a fall one year before.

I have been unable to find any parallel to the manner in which the fragment of bone and the knife-blade gained entrance to the bladder. Considering the size of both, it is absolutely certain that neither of them could have entered through the urethra. They must therefore have worked their way into the bladder through the side wall; and, according to the history of the patient's condition while this was taking place, there certainly could not have been any extravasation or leakage of the urine. Undoubtedly, the foreign body was completely walled off in each case. The bladder must have closed as quickly as the foreign body passed in. My experience with these cases has led me to believe that, in making our diagnoses, we should not

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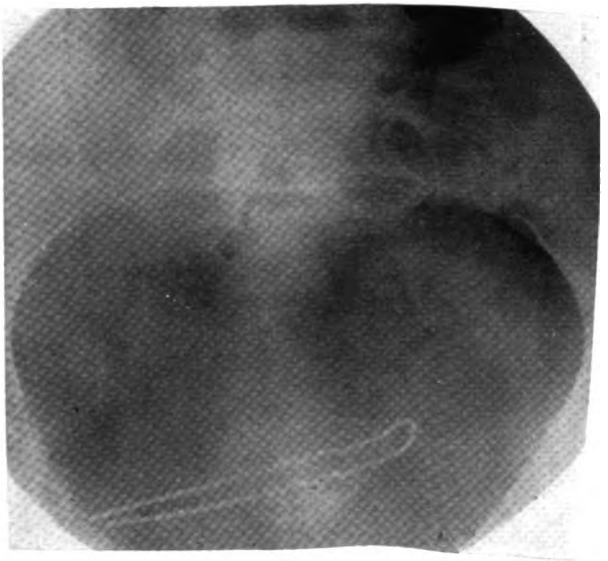

Fig. 1. Hairpin and stony deposit in the bladder. Ends of the pin embedded in the wall of the bladder.

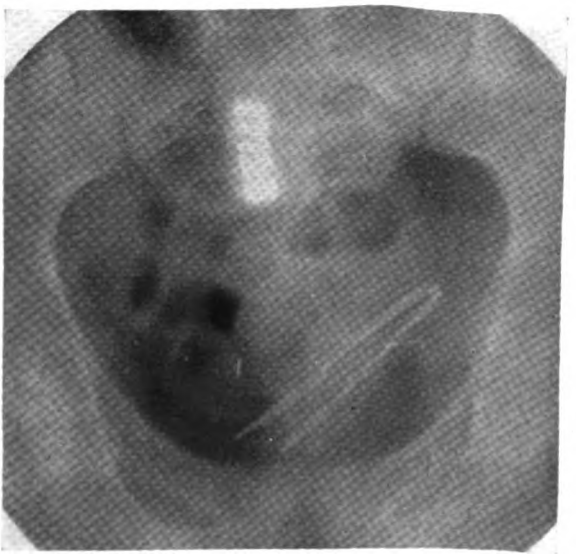

Fig. 2. Hairpin in the bladder with stony deposit about it.

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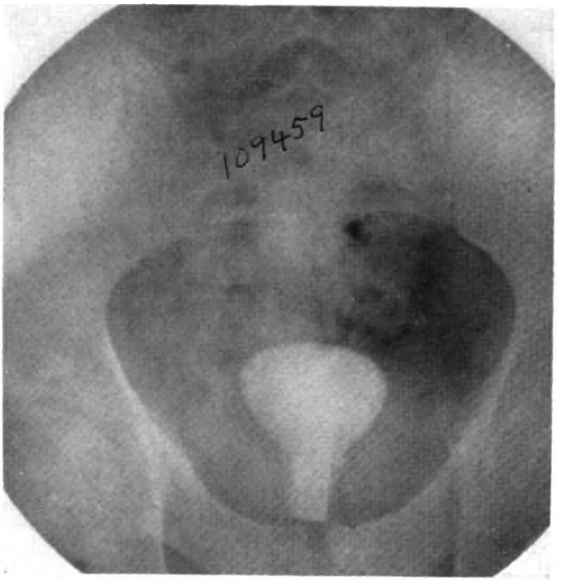

Fig. 3. Large stone in the bladder formed around the blade of a knife. Blade entered the bladder through the side wall.

Fig. 4. Osteomyelitis of the femur. Piece of bone which has been separated lying inside the pelvic cavity. Cystoscopic examination showed the fragment of bone in the bladder.

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rely too much on the history given by the patient, a fact clearly brought out by the röntgen and cystoscopic findings, as noted in this series of cases. Of course, the manner in which some of the foreign bodies entered the bladder is problematical; but, judging from these two cases, I think it may be assumed that foreign objects may pass through the prevesical tissues and the wall of the bladder into the bladder without doing any permanent injury to the wall, or producing serious symptoms at the time they are passing. At least, this is the view which must be accepted until there is better proof to the contrary.

REFERENCES

1. Packard, F. R.: An Analysis of Two Hundred and Twenty-one Cases of Foreign Body Introduced into the Male Bladder per Urethram, with Report of a Recent Case. Annals of Surgery, 1897, xxv, 568-99.
2. Freeman, H. W.: Lancet, 1885, i, 340. Quoted by Packard.
3. Harrison, R.: Lancet, 1885, i, 287. Quoted by Packard.
4. Roberts, A.: Medical Times and Gazette, 1859, xl, 113. Quoted by Packard.
5. Ghose, T. M.: Indian Medical Record, 1893, v, 387 Quoted by Packard.

DISCUSSION

Dr. Oliver J. Fay (Des Moines, Iowa): Dr. Judd's paper is especially interesting to me, perhaps because I can recall only one similar case. A young girl was brought to the hospital with the history of having introduced a pin into the bladder. For some reason the cystoscopist did not succeed in locating the foreign body. As a result, perhaps, of dilatation by the cystoscope, the pin was passed spontaneously, and was found in the bed. Positive identification was possible because the pin was a family heirloom, the gift of a grandmother.

This subject is of interest from a historical point of view, as well. I have an old Italian book on lithotomy by Tommasso Alghisi, published at Florence in 1707, in which several cases of foreign body in the bladder are reported. In one of these a stone had formed about a medicated wick or candle, introduced into the urethra for the treatment of a carbuncle. It was removed through the urethra. In another case, a large stone formed about an ivory handle, in size and shape like an

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embroidery stiletto, which a young girl had introduced into the bladder. It was removed at autopsy, having apparently been the indirect cause of death. In still another case a man suffered from stone in the bladder for twenty-eight years. When the stone was removed at autopsy, its nucleus was found to be a lead ball from a harquebus by which he had been wounded twenty-eight years before.

While in many respects these histories might be of the twentieth, instead of the eighteenth, century, the advent of the *x*-ray and the cystoscope has so simplified the diagnosis of foreign bodies in the bladder that the chapter on diagnosis has been entirely rewritten.

In going from 1707 to 1915, we find that Legueu recently read a paper before the Academy of Medicine of Paris on "The Extraction of Bullets from the Bladder Through the Natural Channels." He called attention to the simplest method of determining whether a bullet shown by the radiograph is located in the bladder, a method which is often overlooked, perhaps because of its very simplicity. The patient's position should be changed and a second *x*-ray taken; if the bullet is free in the bladder it will be found to have changed location. The cystoscope is, of course, the greatest aid in accurate diagnosis, but where no cystoscope is available or where its use is not understood, the taking of two *x*-ray pictures is the best substitute.

Another interesting item from war surgery is the occasional removal of pieces of bone which have penetrated the bladder, although no scar is to be found in the bladder walls.

Dr. Roland Hill (St. Louis, Mo.): A recent case of traumatic injury of the bladder from a bullet will be of interest in this connection. The patient, a man, was in a quarrel, and in running away from his opponent was shot in the right hip above the great trochanter, the bullet apparently being deflected downward, and the house surgeon who saw him thought nothing of it. Several hours later, as the patient had pain in the region of the bladder, we decided on exploration. We found the bullet had gone downward and inward, and had struck the tuberosity of the ischium, had been deflected upward, gone through the pubic bone at the middle, tore it wide apart, and went through and tore up the front of the bladder, where we found it. This was an unusual case of traumatic injury.

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DR. CHARLES H. LEMON (Milwaukee, Wis.): I operated on a woman for the removal of a large stone in the bladder. I had some trouble in removing it. After making traction on the stone I found it was attached to something other than the bladder, and I succeeded in removing the stone, which I found was attached to an ordinary catheter.

Upon making inquiry as to how the catheter got into the bladder, I was told by the patient that eighteen months previously she had gone to a physician to have an abortion performed, and in some way the catheter had been lost. This doctor had evidently pushed the catheter through the uterus, it had lodged in the space of Retzius for eighteen months, and finally perforated through the posterior wall of the bladder, and a stone formed on the catheter.

DR. D. C. BROCKMAN (Ottumwa, Iowa): I desire to report a case in which a very competent surgeon operated on a female for intestinal fistula, left a tube in the bladder, and went away on his vacation. The patient came to me six months later complaining of symptoms of stone in the bladder. I passed a searcher, and found something soft together with the stone, and next day I removed through the urethra a tube of the same kind as he had left in the abdomen.

Another interesting case occurred twenty years ago. Early in the morning I was called to see an old man who had been attempting to pass a soft rubber catheter, and the catheter got away from him. When I saw him the catheter was in the bladder. I passed a searcher, and found a stone. We did a suprapubic prostatectomy, and got the catheter and stone. After introducing the catheter, in going from the bed to a commode, the catheter slipped away from the man, and he went after it. He said he felt it crawling into the bladder. When we took the stone out we found it was triangular in shape, and had caused obstruction. This three-cornered stone that acted as a valve in the bladder had caught the catheter, and when he pulled on it it would not come down. I find nothing like it in the literature.

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END-TO-END SUTURE OF THE BILE-DUCTS

THEODORE F. RIGGS, M.D., F.A.C.S.

PIERRE, SOUTH DAKOTA

The bile-ducts present a field for the most interesting and unexpected surgical opportunities. The literature on the subject, especially the writings of Moynihan, Kehr, Fenger, Mayo, and Jacobson, is most instructive, but anything like a formal review would be unnecessary and out of place in this paper.

The mechanism of obstruction and occlusion of the bile-passages as given by Schueppel (quoted by Fenger), is briefly as follows:

1. Obstruction from within by (*a*) bodies that fill the lumen; (*b*) cicatricial strictures following local inflammations of the mucosa; (*c*) tumors, benign and malignant.

2. Obstruction from without by compression due to masses of scar-tissue or adhesions, the result of chronic inflammation, peritonitis, or perihepatitis.

3. Deviations of the bile-ducts caused by peritonitic adhesions between the hilum of the liver and one or more of the neighboring organs.

Destruction of a portion of the bile-duct may be the result of trauma, gangrenous inflammation and surgical manipulation, either accidental or intentional.

For the relief of the condition present in any given case one of the following recognized methods of repair or reconstruction would probably be used:

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1. Simple suture of the wound.
2. End-to-end anastomosis.
3. Plastic closure or reconstruction by tissue taken from a neighboring viscus, the transplantation of fascia, blood-vessels, or the appendix.
4. Anastomosis of the gall-bladder or bile-duct with the bowel or stomach.
5. Construction of an entirely new duct by the use of a rubber tube covered by omental tissue.

My experience in unusual bile-duct surgery has been limited. In 1906 in Baltimore among seven bile-duct operations on dogs three end-to-end sutures of the lower bile-duct were done with rather unsatisfactory results. Two dogs died on the sixth and one on the seventh day after operation from leakage into the peritoneal cavity. That bile had passed into the bowel in each case was certain, and as nearly as we could tell the sutures had held perfectly until the fourth day. In each case autopsy showed only a slight separation of the line of suture; and, peculiarly, this opening was located in each case at the point uppermost in the field of operation, where one might expect the most accurate suturing to be done. Had drainage been provided for at the time of operation, the dogs might have recovered, but no drains were used for two reasons: first, that we might test the method; and, second, because in other operations where drainage had been provided we had difficulty in preventing the dogs from pulling out the drains. In man this difficulty is not serious; and the value of drainage in all bile-duct operations has been impressed upon us by the highest authorities.

Through the courtesy of Dr. A. W. Elting, of Albany, New York, I shall now report an unpublished case of choledochoduodenostomy in a part of which it was my good fortune to assist. This case is of inter-

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est and value, not only in itself, but also because the obstructing growth seemed similar macroscopically to the growth in the case I shall report later:

Mr. J. B., aged 53, admitted to St. Peter's Hospital, July 31, 1908. Occupation, contractor. He had not had any serious illness except for an attack of malaria at eleven years of age. This lasted six months, and was of the tertian variety. He stated that for the past five years he had bilious attacks occasionally after taking indigestible food, especially cabbage. These bilious attacks were accompanied by nausea, vomiting, and headache. Six weeks prior to admission to the hospital, that is, about June 25, 1908, he first noticed jaundice of his skin and conjunctiva. This jaundice gradually increased and about five days later he noticed that the urine was deeply bile-stained and that the stools were clay-colored. He was sleepy and had marked itching of the skin. He gradually grew worse, and after some three weeks went to Saratoga in the hope of recuperating. While there he grew steadily worse, and began to have fever of an intermittent character with severe irregular chills. The jaundice became very much increased and the general condition very serious, with pronounced toxemia. At no time had there been any history of severe pain or anything simulating colic. He was seen in Saratoga in consultation by Doctor Elting, and was found to be a man fairly well built, although much emaciated, with a very pronounced jaundice. The liver was greatly enlarged, and what appeared to be a very markedly distended gall-bladder could be palpated below the right costal margin. There was slight abdominal distention. The urine contained a large amount of bile with albumin and casts. The appearance was that of a case of obstructive jaundice with marked cholemia.

The patient was operated upon July 31, 1908, at which time a very marked enlargement of the gall-bladder was found. The contents of the gall-bladder were of a mucopurulent character. There were no stones. The pancreas appeared to be somewhat uniformly enlarged with no evidence whatever of any pancreatic new-growth, the appearance being entirely one of chronic pancreatitis. The gall-bladder was drained. The patient did very well after operation, although practically all of the bile was discharged through the wound. The stools remained clay-colored, and there was considerable bile in the urine. The patient left the hospital September 5, 1908, but

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returned on October 16. There had been no diminution in the amount of bile-drainage from the wound, although the skin and mucous membranes had become practically normal in appearance.

The second operation was performed on October 17, 1908, at which time a definite obstruction of the common duct just before it entered the duodenum was demonstrated. The distal inch of the common duct with the papilla was excised, and the proximal portion of the common duct implanted into the duodenum. Drainage of the gall-bladder was continued. The patient withstood the operation very well. At this time there was some slight enlargement of a few lymphatic glands in the vicinity of the hilum of the liver, but no evidence whatever of any neoplasm. The resected common duct presented macroscopically the appearance of scar-tissue. It was carefully examined microscopically at the Bender Laboratory, and reported as a benign stricture of the common bile-duct. The patient made a very satisfactory recovery. The sinus closed fairly promptly and the normal course of the bile was re-established. The patient's health improved greatly, and he left the hospital on November 20, 1908, in excellent general condition, with his wound firmly healed and his functions apparently normal.

A few weeks subsequent to leaving the hospital, he started for Europe. The voyage was an exceedingly rough one, and the patient was very seasick. During this voyage he became somewhat jaundiced, and in the latter part of January, 1909, his wound re-opened spontaneously and began to discharge large quantities of bile. He returned to the United States, and was readmitted to the hospital on February 18, 1909, at which time he was considerably jaundiced. The stools were again clay-colored. There was considerable bile in the urine, some enlargement of the liver, and a profuse discharge of bile through the sinus, which developed in the scar of his previous wound. The patient had lost considerable weight and looked very ill. An irregular mass could be felt in the region of the scar and was believed to be in all probability carcinoma. An exploratory operation was performed and extensive carcinoma of the upper right quadrant of the abdomen was found. This appeared to have originated from the region of the sutured common duct. At this time a re-examination of the portion of common duct excised at the previous operation was made, and after the study of a very

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considerable number of sections, a few sections were obtained in which minute areas of carcinoma simplex were found. This corresponded to the microscopical type of carcinoma as revealed at the operation done in February, 1909. The patient lived for about a week after the operation, and died from exhaustion. An autopsy was not obtained.

The next case is that of a man referred to me by Dr. A. H. Youngs, of Pierre, on November 5, 1912.

C. B. T., aged 54; occupation, assistant in U. S. Land Office. Family history, unimportant; personal history, no serious illness. Indigestion on eating heavy food noted during past twenty years. Present illness began in 1906, associated with an attack of indigestion and characterized by pain in epigastrium, nausea, constipation, and slight jaundice. This was followed by one or two recurrences, similar in character, during the ensuing year, but at no time was the patient seriously ill. Slight attacks were again noticed in 1911 with gradually increasing severity and some loss of weight. A peculiar state of mental depression and increased irritability were noticed by the patient's friends.

Examination showed a large, well-formed man. His temperature was normal; mucous membrane of good color; moderate jaundice and a trace of bile in the urine; stools rather light, but not clay-colored. The general examination was negative, but there was moderate tenderness over the region of the gall-bladder. The liver was not enlarged.

The patient was admitted to St. Mary's Hospital on November 7, 1912, with a diagnosis of cholecystitis and cholelithiasis. Operation November 9, 1912, ether anesthesia. An incision was made over the inner side of the upper right rectus. There were many fine pale adhesions between the gall-bladder and surrounding tissues. The gall-bladder was small, gray in color, and contained a few small palpable stones. No stones were felt in the ducts, but near the juncture of the hepatic and cystic ducts a mass was felt which was thought to be a stone. The field was exposed by packing off the intestines and mobilizing the liver in the manner described by Moynihan, thus bringing the ducts into view. The cystic and hepatic ducts entered a somewhat encapsulated mass of brownish granular material suggestive of an old abscess; and upon clearing this away it was found that the ducts lay parallel for a distance of about 3 cm. The hard substance

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which had previously felt like a stone was now seen to be grayish-white in color, slightly spindle-shaped, and situated exactly at the juncture of the three ducts, involving all three. There was no appreciable dilatation of the hepatic duct. Aspiration of the gall-bladder showed bile-stained mucus. A longitudinal incision of the mass showed an annular constriction almost entirely closing the lumen of the hepatic and common duct, and evidently also obstructing the cystic duct. Macroscopically the mass was about 1.5 cm. long and nearly as thick, and appeared to consist of white fibrous tissue. Malignancy could not be ruled out, but the lymphatic glands found were large and soft, and seemed to be due to an inflammatory process. Because of the possibility of malignancy, and the fact that the gall-bladder could not be used in a cholecystenterostomy, we resected about 3 cm. of the bile-ducts, including the mass, and did a cholecystectomy, all the tissue being removed in one piece. It was first thought to bridge the gap by a rubber tube passed through the common duct to the bowel, but we found it possible to approximate the ends of the severed hepatic and common ducts and accordingly an end-to-end suture was done after the manner devised by Carrel for blood-vessel suture, No. oo chromic catgut being used. The suture line was covered by omental tissue. The hepatic duct was not drained because of possible valve-formation from infolding of the edge of the common duct, but a rolled rubber-tissue drain was placed near, but not touching, the site of the anastomosis; and, after covering in the raw surface of the liver, left by the removal of the gall-bladder, the abdominal incision was closed. The patient withstood the operation well. The bowels were moved freely on the third day, bile showing plainly in the stools. On the third day the patient vomited several times and was troubled by severe hiccough but there was no bile on the dressings. After a very restless night, because of the hiccough, a faint stain of bile was noticed the morning of the fourth day, November 12. The biliary drainage gradually increased in amount for several days, and on November 16 the stools were clay-colored, but soon the bile appeared again in the bowel movements and on December 9, 1912, after having been up since November 26, the patient was discharged. Microscopic examination of the constricting mass showed only scar-tissue, and the fact that today, three years after the operation, the man is in excellent health would bear out

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this report. I regret that owing to an accident two years ago which destroyed an entire tray of specimens I am unable to show you this specimen.

In September, 1914, J. H. Jacobson, of Toledo, Ohio, in an excellent paper entitled "Repair and Reconstruction of the Bile-Ducts" reported an end-to-end anastomosis of the severed ducts. He carefully reviewed the literature and collected thirty-one additional cases which he classified as follows under the head of "Operative Technique Employed."

End-to-end anastomosis with circular suture, with drainage of the hepatic duct	21
End-to-end anastomosis with circular suture, without drainage of the hepatic duct	2
New duct formed from loop of small intestine	3
New duct formed by rubber tube	2
Hepaticocholangioduodenostomy	1
Cholecystocholedocostomy	1
Plastic closure	3
Not stated	1
	34

In this series there were only two deaths.

In Jacobson's case bile appeared on the dressings on the fifth day, and in reality his case and the one here reported were little more than modifications of end-to-end anastomosis with hepatic drainage.

That in approximately 90 per cent of the cases reported injury to the bile-ducts was accidental should serve to emphasize both the frequency of atypical implantation of the cystic duct and the necessity of extreme care in the technic of cholecystectomy. No doubt we have all heard Dr. C. H. Mayo say that "the gall-bladder should never be used as a handle," but that one should begin at the cystic duct in doing a cholecystectomy.

It would be interesting to compare the possible advantages of the method of Sullivan, Stone, Walton,

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Lanphear, or R. H. Jackson over that which was used in this case, but as this is impossible I present the report as it is, appreciating fully the words of Dr. George Emerson Brewer relative to certain surgical successes: "When aided by nature, a sound constitution, and a high degree of normal resistance."

REFERENCES

Brewer, George Emerson: Some Observations upon the Surgery of the Biliary Passages. Surg., Gynec. & Obst., 1912, xiv, 433.

Davis, Carl B., and Lewis, Dean: Repair of the Common Duct by Means of Transplanted Fascia. Tr. Western Surg. Ass., 1913, p. 217.

Elting, Arthur W.: Choledochoduodenostomy. Personal Communication.

Fenger, Christian: Retention from Displacement, Bending, and Valve-formation (Oblique Insertion) in the Biliary Tract. Collected Works, vol. ii, p. 815.

Jackson, Reginald H.: Anterior Choledochojejunostomy. Surg., Gynec. & Obst., 1914, xix, 232.

Jacobson, J. H.: Repair and Reconstruction of the Bile-Ducts. Am. J. Obst., N. Y., 1914, lxx, No. 6.

Lanphear, Emory: Two Operations for Total Destruction of the Gall-Ducts. Surg., Gynec. & Obst., 1909, viii, 406.

Mann, Arthur T.: A Rubber Tube in the Reconstruction of an Obliterated Bile-Duct. Surg., Gynec. & Obst., 1914, xviii, 326.

Mayo, Wm. J. and Chas. H.: Complete Obstruction of the Common Duct of the Liver; Anastomosis between Gall-Bladder and Jejunum by Means of Murphy's Button. Northwestern Lancet, 1893, June 1.

Mayo, Wm. J.: Some Observations on Cases Involving Operative Loss of Continuity of the Common Bile-Duct; with the Report of a Case of Anastomosis between the Hepatic Duct and the Duodenum. Ann. Surg., Phila., 1905, July.

Moynihan, Sir Berkeley: Operations for Obstruction of the Common Duct. Abdominal Operations, vol. ii, p. 308.

Stone, I. S.: Complete Division of the Common Duct at Junction with Hepatic and Cystic Ducts, Suture. Surg., Gynec. & Obst., 1909, ix, 590.

Sullivan, Arthur G.: Reconstruction of the Bile-Ducts. J. Am. M. Ass., 1909, liii, 774.

Walton, Albert J.: Reconstruction of the Common Bile-Duct. Surg., Gynec & Obst., 1915, xxi, 269.

DISCUSSION

Dr. A. C. Stokes (Omaha, Neb.): I think the doctor is to be congratulated on his very excellent paper and on the very splendid results, because gall-duct anastomosis is to me one of the difficulties that I am always afraid of, and one of the things I am always trying to avoid, if possible.

It has occurred to me that there are two points in the biliary ducts that are the most susceptible to disease, and that almost all anastomoses that have for their object the uniting of bile-ducts should be made with respect to these two ends: first, junction of the cystic duct with the hepatic duct; and,

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second, the distance of the common duct from the time it enters the wall of the duodenum. Those are the parts of the ducts where most injuries occur either from operative intervention or from pathologic growths. These are the parts of the ducts on which experiments have been made and are being made to determine the method of anastomosis.

The technical difficulties are extraordinary. The bile-ducts will unite themselves very readily if only approximated at any point and if there is no obstruction. It does not take much more than one or possibly two or three stitches to bring the posterior ends of the bile-ducts together until they unite if there is no pressure or any pulling to pull the stitches out and pull the bile-ducts apart. It is surprising to me how they unite so readily at times.

The difficulties connected with the liver in end-to-end anastomosis are those described by the doctor. If we find a big mass we have to dissect it up. The surprising thing to me is that he was able to hold the hepatic duct so as to draw the thing together. I have seen it divided into two parts, and did not know which was the best part to anastomose, although in these cases we can usually bring the common duct up to the liver and unite it to something; and I have noticed in the literature that half of the cases recover.

Putting the common duct into the duodenum has been done a great many times, but not over 40 per cent of them got well and remained well any length of time. I do not think anastomosis of the common duct with the duodenum by passing it in a new area is any more successful than other methods.

Dr. L. L. McArthur (Chicago, Ill.): I would like to say a word in regard to this subject because I had to meet an emergency at one time under distressing circumstances. The truth of the matter is, that one does not get strictures of the common duct to operate on until very late—until many efforts have been made, as a rule, to relieve them by other means, and when traumatic in origin there is no distal portion to make a suture to. However, in end-to-end anastomosis and coming to the utilization of what remains of the common duct, I have succeeded in making a new common duct, and I think the dates will show I was the first to do that in a case of this kind.

Our good friend, Dr. Mayo, removed stones from the gall-bladder for a patient, but the gall-bladder refused to heal: it left a fistulous tract. He removed the gall-bladder. This refused to heal, the biliary flow persisting. Whenever it did heal,

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jaundice would occur. He made a third operation for the purpose of removing the obstruction in the common duct, but the difficulties incident to the adhesions at the second and third operations were so great that it became necessary to apply forceps to check hemorrhage, and he put the patient back to bed. After getting over the shock and recovering with a fistula, he refused to go back to Dr. Mayo, which was a mistake, though I had the temerity to attempt the cure after my esteemed colleague. After mining through these adhesions I found a sacculated common duct on the hepatic side on the proximal end, but there was no distal end to be found. To make a new drainage-point or channel I made a hole in the sac, and succeeded in putting in a piece of rubber tubing about six inches long, on the proximal end of which I turned back two reveres. I made a hole in the duodenum, put the tube through till it hung about six inches down the duodenum, and I then inserted a purse-string suture through the wall of the duodenum, and drained the wound in the line of old incision. In that way I maintained a new drainage-channel, and the patient recovered, passing the tube at the end of nine weeks per rectum.

This operation was done before Sullivan made his report, which is confirmatory of it; and it was done three years before the meeting of the Clinical Society in Heidelberg when Wilms showed us a new procedure, which was the same thing without the opening. I have done the operation successfully three times since.

Dr. Arthur T. Mann (Minneapolis, Minn.): As I reported a case two years ago, I think a subsequent report of it may be of some interest. Infection of the bile-ducts and liver from back-flow from the duodenum is one of the great drawbacks to this operation. If the common duct were large enough and could be sutured, this would preserve the normal papilla in the duodenum, and prevent such infection. Often this cannot be done, and, I think, in that case we should try to construct a papilla.

In my case the lower part of the common duct was destroyed by necrosis following infection, and I reconstructed the opening into the duodenum, using the rubber tube and inverting the bowel by three circular stitches, as in the Kader-Senn operation for gastrostomy. This formed an artificial papilla, which I hoped would close against the peristaltic pressure of the duodenum, and in this way prevent back flushing into the

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bile-ducts and duct-infection. How well I succeeded I do not know, but I think this is an important point in the artificial reconstruction of the common duct.

Of course, the tube can be run through at a slant, under the mucous membrane, and the mucous membrane be pierced below the hole through the muscular layers of the duodenum. This gives a tubular, slanting opening which may close under the pressure of peristalsis; but it must be lined by mucous membrane before it can be permanent. Therefore the tube must be fastened so that it will last long enough to allow the mucous membrane to grow up from the duodenum, and down from the bile-duct, to line it. The same thing may be done by infolding the duodenum about the rubber tube, as is done in Witzel's gastrostomy. A fascial transplant, an omental transplant, or the tissue in the immediate neighborhood,—any one of them,—may be used at times to fold about the exposed part of the rubber tube to help reconstruct the duct.

In order to avoid the necessity of reconstructing a bile-duct, it is important to keep in mind the anatomic anomalies of the ducts during our ordinary gall-bladder and gall-stone operations. The hepatic duct, the cystic duct, and the common duct are not always in the same relation. I have seen the right hepatic duct enter the common duct below the cystic duct; and I have seen it running along in the same sheath with the cystic duct. There are all sorts of variations, and we must be careful in the dissection of our parts in the original operations.

As to the subsequent history of my case: From time to time there was a mild infection back from the duodenum into the bile-ducts. The patient had mild attacks of jaundice with the symptoms which go with it, and then she would clear up. I saw her last fall. She came into the office smiling, said she had gained twenty-four pounds in weight, and felt better than she had for years.

Dr. Charles H. Mayo (Rochester, Minn.): I really had not intended to speak on this subject, but since Dr. McArthur has referred to a case on which I operated I will say a few words. This is a most difficult line of surgery, and I can think of some other cases, besides the one related by Dr. McArthur, in which I had a lot of trouble. Dr. Riggs was very fortunate in his case, for the reason that he was able to complete his work at the primary operation. There is nothing more difficult than the secondary and tertiary operations, as in the case related by Dr. McArthur. I saw some of Wilms'

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cases in Heidelberg. He had three patients in whom tubes connected the hepatic duct with the duodenum. *X*-ray pictures of three of his cases showed the tubes which had been in place for one and two years.

A soft T tube, the size of a catheter, welded together, is a useful accessory for common-duct work. In cases of the type reported by Dr. Riggs we have used a number of these. We pass one end of the T tube through the lower end of the duct into the bowel; and the other end into the hepatic duct.

Last summer I had three of these cases, in which I used the ends of the catheters as a Sullivan tube. The bell-shaped male catheter is one of the best things to use. I leave these tubes in a considerable time, cutting them off to the right length, and putting the bell up into the dilated hepatic duct, where it will hold for a longer time. If one finds the pylorus and the stomach bound down by the common duct as the result of a previous operative procedure or by inflammation, instead of trying to loosen the duodenum to get the stomach up, one of the best things to do is to establish permanent drainage just above the pylorus into the stomach. These patients will not complain that there is any trouble from bile. A number of operators in Europe, where it is possible to use the duodenum or stomach, make it a rule to unite to the stomach.

Dr. Daniel N. Eisendrath (Chicago, Ill.): Dr. Mayo has brought out one point with reference to anomalies which I think is of great importance. I do not think we can peruse any chapter in any book with more profit than the chapter on anomalies of the cystic and hepatic ducts and of the arteries around the ducts in Kehr's book on "Gall-Bladder Surgery."

One point mentioned by Dr. Riggs, namely, the parallelism of the ducts, is not near as infrequent as we are apt to think. The cystic and hepatic ducts normally meet each other at an acute angle. That is rather an exception than the rule, and for that reason it is not uncommon for those who do not know of anomalies in the anatomy to injure the common duct by clamping it at times. The same is true of anomalies of the cystic artery which are quite frequent. An anomalous artery occurs, which is very large, and comes up around the back of the common duct in such a way that we are apt to clamp the common duct by accident.

I would like to ask Dr. Riggs whether there has been any recurrence of the symptoms of sepsis or any jaundice?

The usual experience in end-to-end anastomosis of mucous-

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membrane lined tubes, like the ureter, is secondary contraction with stricture formation, and I would like to ask whether anything of that kind occurred in his case.

DR. RIGGS (closing): Answering the question of Dr. Eisendrath, I will say that immediately following the operation the jaundice cleared up rapidly, and the patient has remained free from it ever since. The man feels as well as he ever did. The symptoms of indigestion which he used to have have disappeared; and he has gained fifteen or twenty pounds in weight, and is working every day.

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NAILS AND SCREWS IN JOINT SURFACES

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This series of experiments was undertaken after nailing a fractured and misplaced external condyle in the elbow of a boy of seven, through the joint surface, in order to determine the behavior of nails and screws so placed, and the joint changes in response to their presence.

This boy was referred to me six weeks after the fracture with the fragment still out of place, though the usual methods had been faithfully used to replace it. I was unable to replace it by any of the ordinary methods, and determined upon an open operation. After opening the joint we found the fragment rotated nearly one hundred and eighty degrees about a slight attachment to the external lateral ligament of the elbow and standing straight out from the joint. Little or no callus had formed, and it was fairly easy to reduce it, but it would not stay in place. It could not be made to stay in place with the elbow either at acute-angle flexion or at right angles, or with the arm straight. The head of the radius forced it out of place. We decided to nail it through the joint surface. This was easily done. Firm adhesions developed to the tissues about the joint, which, later, had to be broken up forcibly under ether. This loosened the fragment somewhat from its bed, but the final result was a good serviceable arm with moderate limitation at the extremes of motion.

The method described was suggested to me by an admirable article on joint fractures by Sherman and Tait in the course of which they detailed some experiments with Lane's plates, bone pegs, screws, and

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staples, and in the course of which they stated that, after a painstaking search, they could find no case in the literature in which nails and screws had been used through the joint surfaces.

I think my series of experiments bring out some further points of interest.

In the whole series no joint became infected, and no specimen was lost. In most of them rubber gloves were used; a few were done with the bare hands to see if the technic would stand this amount of exposure. No dressings were used after painting the suture line with tincture of iodine, and the dogs were allowed to run about without fixation of the joints. Most of them favored the leg operated on for a time, but, as a rule, they soon lost their limp, and all of them were running about actively without a limp at the time the specimens were recovered.

Dog No. 690.—*Right knee:* Two nails were driven into the inner condyle, and countersunk less than the thickness of the cartilage. Moderate trauma was added to the cartilage about them.

Four months and six days after operation (Fig. 1): The nails are entirely covered with a thin layer of tissue apparently of the same color as the rest of the cartilage. The cartilage is nearly smooth, but not quite in the area close about the nails. The semilunars and the joint cartilages are otherwise apparently smooth and normal, opposite the nails as elsewhere. There is no staining at the heads of the crucial ligaments.

Dog No. 690.—*Left knee:* A five-eighths inch silver-plated screw was countersunk into the inner condyle of the femur well below the thickness of the cartilage. A slight amount of blood was in the joint.

Three months less two days after operation (Fig. 2): The screw is entirely covered, and the condyle apparently *slightly built up about the screw area.* The covering is not yet quite smooth, and does not show quite as clear a translucent gray as the rest of the cartilage.

The cartilage on the outer posterior quarter of the other

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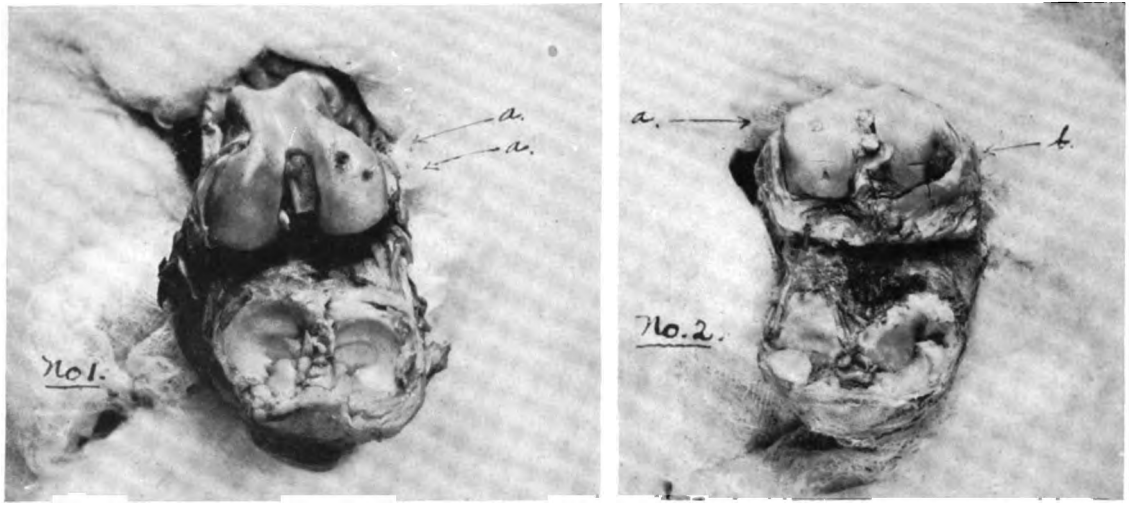

Fig. 1. Experiment 690, right knee, 4 months 6 days after operation. Two nails countersunk less than the thickness of the cartilage. *a.* Nails entirely covered with connective-tissue.

Fig. 2. Experiment 690, left knee, 3 months less 2 days after operation. Five-eighths inch silver-plated screw sunk well below cartilage surface. *a.* Screw entirely covered. Condyle apparently slightly built up about area of screw.

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condyle shows some thinning, is a pale pink, and the surface is not quite so smooth as the normal cartilage, possibly from overuse due to a faulty position of holding the leg during the stages of convalescence. This was the only case in which changes suggestive of osteo-arthritis were present. The cartilage of the tibial head suggests a very slight similar change, while the semilunar seems normal. This was an old, poorly nourished dog, and we came to the conclusion that this was probably present at the time of the experiment, and not the result of the experiment.

The surfaces opposite the screw seem normal. Though some blood was left in this joint the roots of the crucial ligaments are not as stained as in most of the other specimens, practically none at the femoral ends.

Dog No. 697.—*Right knee:* Two nails were driven into the internal condyle of the right femur and countersunk well below the depth of the cartilage.

Three months and twenty-four days after operation (Figs. 3 and 4): The nails are completely covered, and the tissue is almost level with the joint cartilage, slightly pitted but nearly smooth. The semilunar cartilage and the bearing surface of the tibia opposite the nails are perfectly smooth and normal. *The cut surface* shows no apparent overbuilding of bone or cartilage above the normal level. Both nail-heads are covered across with bone, which is lined with tissue about the same thinness as the joint cartilage.

Dog No. 697.—*Left knee:* One-half inch silver-plated screw was countersunk, not quite the thickness of the cartilage, into the condyle of the left femur. A second larger drill was used to take care of the flare of the screw head.

Two months and twenty days after operation (Figs. 5 and 6): There has evidently been *a slight upbuilding of the condyle* about the screw. The screw-head is entirely covered by a thin plate of tissue, at a lower level than the joint surface and with the lower edge growing over the edge of the screw with almost the joint level. The opposing joint surface on tibial and semilunar cartilages is smooth and normal. *The cut surface* shows bone beginning to grow inward over the top of the screw, and is covered in turn by a thin cartilaginous or connective tissue.

Dog No. 726.—*Left knee:* A silver-plated screw was countersunk into the inner condyle of the left femur so that one

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edge was flush with the surface, and the other edge buried one and one-half millimeters below the surface.

Six months less two days after operation (Figs. 7 and 8): *The surface of the condyle has built up* so that the free edge is buried. The edges of the tissue have grown over the screw-head, burying all but one-fourth of its area, and have grown way across in the groove of the screw. The joint surface is glossy and smooth at all other places in the joint. There is no roughness or inflammation of the head of the tibia or semilunar cartilage opposite. The joint is apparently normal in every other way, except possibly the slight staining at the head of the inner crucial ligament. *The cut surface* shows bone as well as connective tissue in the tissue growing over the edge of the screw-head, and the advancing tissue is not quite as thick as the normal cartilage of the joint.

Dog No. 726.—*Right knee:* A steel screw was inserted as before so that its upper edge projected over 0.5 millimeter, the lower edge being buried about 1 millimeter, the inner edge flush with the cartilage, and the outer edge about 0.75 millimeter below the free surface. Five months after operation (Fig. 9): *The surface of the condyle is built up* enough to more than make up for the projection of the screw. The edge of the screw is covered to a very slight extent three-fourths of its circumference, although the inner quarter is still free and uncovered. The joint is otherwise smooth and apparently normal, with no scratching of the tibial cartilage or the semilunar over which it swung and got its bearing, except that a little gloss is lost on the surface. There is a slight staining at the roots of the crucial ligaments at the femoral end. (The condyle was not sawed open.)

Dog No. 576.—*Right knee:* Two nails were driven into the same condyle of right femur, one anterior to the other. The anterior one, a small head nail, was driven in flush with the cartilage surface. The second was broken off below the head and the jagged edge was left projecting about 2 millimeters above the joint surface. The joint was freely swung back and forth to cut a groove into the head of the tibia and into the semilunar cartilage.

Six weeks plus one day (Figs. 10 and 11): Undoubted *growth in thickness* over the major portion of the condyle about the region of the nails, to an elevation represented by the projection of the broken nail. Neither nail surface is

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entirely covered. The new tissue is nearly smooth and apparently of the same character as the normal cartilage, but in reality shows a connective-tissue structure. The groove, which had been cut deeply through the semilunar cartilage, the cartilage of the tibia head, and into the bone, has smoothed up and partially refilled in the six weeks. The surface is covered smoothly with cartilage, looking like the normal cartilage of the joint surface. The joint cartilages are otherwise smooth and normal. There is no staining at the heads of the crucial ligaments. *The cut surface:* The new increase in tissue seems entirely due to new bone. The new cartilage which covers this elevated area is no thicker than the thin cartilage of the joint surface.

CONCLUSIONS

1. Nails and screws are tolerated in joint surfaces in the human as well as in the experimental cases, and with surprisingly little reaction.

2. They remained firmly imbedded in every specimen recovered.

3. In every case where the nails and screws projected above the joint surface, there was a distinct upbuilding of the condyle as nature's reply to a rigid metal body projecting into the joint.

4. It is exceedingly interesting to find that the increase in joint level seems always due to the growth of bone and not to the increase in the thickness of the cartilage.

5. In each case where tissue grew across over the head of a nail or screw, or across in the groove of the screw, the new tissue showed a reversion to the connective-tissue type.

6. Even when the nails and screws have remained more or less uncovered, the dogs have run about normally after a short convalescence.

7. In each case the scratch or groove on the opposing surfaces was filled in as the projection of the nail or screw lessened by the upbuilding of the condyle.

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The defect was apparently entirely filled in all but one specimen and this was closing in nicely at the end of six weeks.

8. As a point in technic it seems better to swing a hinge-joint freely at the time of the operation in order to scratch the groove made necessary by a badly placed nail or screw, and thus save the time and pain during convalescence which would be required in scratching the groove little by little later.

DISCUSSION

Dr. J. Frank Corbett (Minneapolis, Minn.): Often when we put a Lane plate on the shaft of a long bone the shaft of that bone begins to thicken, and may actually become heavier. When we come to analyze this we find the thickening is due to a deposit of subperiosteal bone,—that is, there is a new growth of bone between the old shaft and the periosteum. That condtion seems to be somewhat analogous to what occurs beneath the cartilage when a screw is placed in a bone through the joint surface. In looking over Dr. Mann's specimens I could not find any evidence of hyperplasia of the cartilage. The cartilage appears to be pushed forward by a growth of subchondral bone. This compensatory growth of bone, probably, is tendered possible by the abundant blood-supply occurring in the neighborhood of joints. In this connection it is of interest to remember Lexer's teachings concerning the blood-supply of bone. In the long bone Lexer gives a four-fold supply: first, the periosteal vessels; second, the nutrient vessels entering the shaft of the bone; third, the epiphyseal vessels; and, fourth, parepiphyseal vessels. Macewen has emphasized this in his experiments, where he destroyed the ordinary nutrient artery entering the shaft of the bone. When this was done sufficient blood from the epiphyseal and parepiphyseal vessels entered the bone to keep the entire structure alive. Therefore, the ends of the long bones are abundantly supplied with blood-vessels, and we further know that in the adult these blood-vessels have a free anastomosis with each other.

Dr. J. W. Andrews (Mankato, Minn.): I have been very much interested in Dr. Mann's paper. A paper of this kind before a company of surgeons as able as I am addressing now is a very useful one; but, gentlemen, those of you who do not

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practice medicine in the country, or come into touch with the country, do not know how much harm has resulted from the use of foreign bodies or from the use of Lane's plates, screws, and nails, in or near joints or in the shaft of the bone far removed from the joint. I have in mind now one surgeon—and a good man—who used a Lane plate and got infection, so that the leg is practically useless, and he has been sued for $20,000.

I do not understand that, in this presentation of Dr. Mann's and his experiments on dogs, he recommends, or would it be necessary ever, to leave a nail or foreign body projecting into the joint, and hence the necessity of making a groove for the head of the nail. Probably Dr. Mann in his summing up will answer that point.

I have myself, in a number of cases, used silver wires and nails in the treatment of fractures. In very few cases have I used the Lane splint or plate. I have been fortunate where I did use a Lane splint in getting fairly good results, but I have had to remove several Lane splints that were put in by other surgeons, and when we consider the thickening of the bone in the presence of this foreign body and how hard it is sometimes to get union, it seems to me it is unwise to use a Lane splint. Where we use silver wire or a small nail coated or plated with silver, as in the case of Dr. Mann, it is quite a different proposition; but, with reference to the Lane splint, I do not hesitate to say that it has done more harm in surgery than it has done good, and in my own practice I would hesitate to use it unless the necessity for its use became very great.

Dr. Clarence M. Nicholson (St. Louis, Mo.): I have had two cases of intracapsular fracture of the neck of the femur in which nails were used to hold the fragments in apposition.

As shown by the radiographs, the end of the nail, in the first instance, passed through the cartilage about one-sixteenth of an inch into the joint cavity, but, instead of being covered over, as in the experiment of Dr. Mann, an aseptic rarefying osteitis followed.

In the second instance, the point of the nail did not enter the joint, though, as in the first case, the nail loosened. Following the removal of the nail and the substitution of a piece of bone from the shaft of the tibia, both fractures united, and both patients recovered with useful limbs.

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I have held fractures of extremities of bones in position by the use of screws passed into the fragment through the extremity of a Lane plate, the plate having been secured to the shaft of the bone. In such instances, where the blood-supply was greatly interfered with, I have had a rarefying osteitis and failure of union result, though I have repeatedly plated the clavicle, radius, humerus, femur, and tibia, where the blood-supply was not greatly interfered with, and as yet have not found it necessary to remove either a screw or a bone plate.

DR. CHARLES H. LEMON (Milwaukee, Wis.): I am not familiar with the experimental work of Dr. Sherman, but I have had some experiments with the use of nails in joints, and for that reason I rise to ask Dr. Mann why experiments have been made by introducing nails through joint surfaces when we would never meet this condition in surgical practice. He has reported a case where experimentally he cut into the joint, putting in nails and screws without there being a fracture of the condyle. Is that correct?

DR. MANN: Yes, so far as it goes.

DR. LEMON: I hardly see how we can draw a correct conclusion from this experimental work as to what would happen to nails in joints in which the nails are driven through broken fragments. In this latter case we have to deal with the pull made by the muscles, and my experience with fractures of the epiphysis, and particularly of the internal malleolus, is, that, when we drive nails through the condyles, the greater part of which is cancellous tissue, we can almost push in a screw or a nail, because the tissue is so yielding.

I think Dr. Mann would do a great service if he conducted a series of experiments in fractures of the condyles to see what would happen to the screws and nails, which are used to fasten them in place when the dog runs around; and, in addition to the weight that is placed on the joint, we have the problem of the constant pull of the muscles on either side of the joint.

DR. DANIEL N. EISENDRATH (Chicago, Ill.): I want to refer to two cases with the type of fractures that Dr. Lemon mentioned. One was an old woman, eighty-three years of age, and the other a woman, eighty-nine years of age. Both had supracondyloid fractures of the femur. One had the so-called T-shaped fracture of the lower end of the femur, the bone

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being split apart, and one fragment being driven almost straight down into the joint, completely separating the condyle. In that case there was no choice except to put a screw or plate into the joint, and that was done. The plate healed in. The patient died at the age of ninety-two, and during the remaining three years of her life there was no disturbance of the joint. The same was true of the woman of eighty-three, and she had excellent motion in the joint, showing it is necessary to make application of these experiments to human beings as well as to animals.

DR. FREDERICK A. BESLEY (Chicago, Ill.): I rise to make the point that Dr. Mann is dealing with bone in which the circulation is perfect, and that the nail going through the surface of the joint is in bone in which the circulation is normal. In using nails or screws in fractures, we are dealing with fragments of bone in which there is no circulation practically; and it does not seem to me that the conditions are analogous. The important point is the question of what effect nails have in bone in which the circulation is poor.

DR. WILLIAM FULLER (Chicago, Ill.): When Dr. Mann proceeds to carry out experiments as suggested by Dr. Binnie he will find that muscle-pull will have no effect on the subsequent loosening of the screws, provided the latter have been placed in the bone under proper conditions. He will get precisely the same results as he has shown here where no muscle-pull has been exerted.

Within a month I have had skiagrams made of Lane plates introduced over two years ago. The metal plates are shown to be not only covered by bone, but bone can be seen between the threads of the screws. Ever so great a pull, whether by muscle or pulley, will have no effect upon the screws if the latter have been introduced in a proper manner.

The statements we so often hear about the early loosening of screws inserted in bone are true; but the reasons are never given. If screws are inserted into bone after boring holes that are too large, as is often done, and that under a technic of an inferior order, the screws thus inserted will not hold. It does not require a week nor a day, but a few moments only, for the screws to loosen and come out.

Screws placed in healthy bone under the strictest surgical cleanliness with the slightest possible injury and trauma to the soft parts, and into holes not too large, will hold indefinite-

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ly, and, instead of working loose and coming out, will, as time goes on, become more and more secure.

DR. MANN (closing): I feel very much gratified at the free discussion of my paper. This series of experiments must be considered as a preliminary report, given for the points the experiments show. The question of the use of Lane plates is an exceedingly interesting one, but is not relevant to the points given in the paper.

The joints were not immobilized, and the only quiet they received was what the dogs gave them. After the early periods of convalescence, all of the dogs walked without a noticeable limp. I think Dr. Corbett will vouch for this, and it corresponds to the statement of my two laboratory assistants. I have not relied upon my own judgment alone, but I have tried to be fair in presenting to you what these experiments show.

The other proposition in connection with fractures, namely, the loss of blood-supply to the fragment, is very important, but such work as that based on these experiments must be the basis for future work in the human, thus placing us in a better position to judge of what can and what cannot be accomplished.

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OSTEOMYELITIS INVOLVING THE HIP-JOINT, A CONDITION HERETOFORE ERRONEOUSLY DESIGNATED ACUTE EPIPHYSITIS

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Acute suppurative inflammations affecting the joints of young children are grouped under the following names: acute suppurative arthritis of infants; epiphysitis; and acute ostitis of growing bone.

An acute suppurative synovitis occurs in children under four years, which Krause has found to be due to the presence of streptococcus pyogenes. It may be excited by injury or it may occur with the exanthemata. It gives the symptoms of a phlegmon, and usually yields promptly to free incision and drainage. It attacks the joints of the hip, knee, shoulder, and elbow.

There is another disease, usually placed under this heading, that is of a graver character than the above described disease, which begins at or near the epiphyseal line. It occurs in the hip-joint, and has been well described by Macnamara under the name "epiphysitis." *It is an acute osteomyelitis.* The symptoms all point toward an acute inflammation of the hip-joint. There is excruciating pain and high temperature, followed by deformity and swelling. The joint is so deep-seated that redness does not occur. The symp-

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toms are much more acute than in tubercular ostitis of the hip. There is apt to be an early separation of the epiphysis, with rapid destruction of the hip-joint. The treatment consists of free incision and drainage. The opening should be made large enough to admit a finger, in order to ascertain whether the epiphysis is separated or not. If separation has taken place, the epiphysis should be removed at once. This is a grave disease, but with prompt treatment the writer has seen cases recover that were seemingly beyond help.

The above description was published by the writer eighteen years ago, and is just as good now as it was then. It should be observed that at that time the writer did not accept the commonly accepted term "epiphysitis," but pronounced it an acute osteomyelitis. Constant observation since that time has only confirmed the opinion then expressed, that this disease does not begin in the epiphysis, but does begin in the neck of the femur; and one case cited goes to prove that it may even begin in the shaft of the femur. The epiphysis is the favorite site for attack by the tubercle bacillus, but there is no evidence whatever that this is ever the original point of infection from pyogenic germs. The reason the name "epiphysitis," originally suggested by Macnamara over twenty-five years ago, has been and still is in vogue is that osteomyelitis is quite rare in this location, and the diagnosis is not made until the joint has become involved and the epiphysis has separated from the neck. The epiphyseal line is the point of least resistance and soon gives way, and the epiphysis becomes separated. When the joint is opened the floating epiphysis at once attracts the operator's attention, and the original opening into the neck of the femur can be easily overlooked. Some reported cases of extremely rapid recovery after excision of the hip for tuberculosis have not been tuber-

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culosis, but have been cases of osteomyelitis with separation of the epiphysis. Unfortunately, the infection in these cases is usually so severe that unless drainage is promptly established the patient loses his life; but in the exceptional case the infection is less virulent and Nature may establish drainage by rupturing the abscess through the skin. The separated epiphysis acts as a foreign body, and sinuses remain open. Deformity takes place, and the case assumes the appearance of the common tubercular hip, so that it may not be possible to make a differential diagnosis until an operation has been performed and a careful bacteriologic examination has been made.

Of the many cases of osteomyelitis involving the hip-joint seen by the writer, only four will be presented as proof of the correctness of the statements made in this paper.

Case 1.—A boy, six years of age, was admitted to the University Hospital with a very high temperature and in a semicomatose condition. His right limb was everted, and the thigh very much swollen. The attending physician stated that the trouble began a few days before his admission, with high fever and very severe pain in the hip. A diagnosis of acute osteomyelitis was made, and an incision made on the outer aspect of the thigh, as the boy's condition precluded any radical procedures at that time. Drainage was very profuse, and rapid improvement followed, so that in a few days the patient was anesthetized and the original opening enlarged. Through this opening a finger could be passed through an opening in the lower wall of the capsule of the hip-joint. The joint was then opened. The epiphysis was found separated, and the whole neck of the femur destroyed, so that it was necessary to make a formal resection of the hip-joint, after which improvement was rapid. The epiphysis was not much eroded, but the neck of the femur was broken down and necrotic, showing that the latter was undoubtedly the original seat of infection.

Case 2.—Hulda M., aged 9, was admitted to the University Hospital August 4, 1915, under a diagnosis of

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acute articular rheumatism. She gives a history of having had a fall one month before admission, from which she suffered very little. Two weeks before admission she began to have pain in the joints, was nauseated, and vomited. Since that time she had repeated chills. Most of the pain was in the upper part of the left femur. She gave the appearance of being profoundly septic.

She was first admitted to the Department of Pediatrics, where it was found she was suffering from an empyema of the left side and an abscess just below the left breast. She was transferred to the surgical service on August 7, when she was found to have in addition to her empyema an osteomyelitis of the upper third of the left femur and some swelling of the left wrist, and an abscess in the anterior chest-wall. A section of one rib was removed from the axillary line, and drainage of the pleural cavity was established. The abscess of the chest-wall was drained. Very little anesthetic was given, operation was performed in a very few minutes, and the patient seemed no worse for it. Her temperature dropped immediately, but she still was very ill, giving evidences of profound sepsis.

A definite focus in the upper third of the left femur could be located with very little difficulty from the periosteal tenderness. There was some swelling of the hip-joint, but with care the joint could be moved without giving pain. There was flexion of the hip-joint. It was impossible to determine which was the primary focus, the femur or the empyema, but very probably the former. Notwithstanding the drop in temperature her condition was so low that we did not dare give an anesthetic for operation upon the femur. When she was admitted, her temperature was 103° and her pulse 160. After the operation her temperature dropped immediately to normal; pulse remained at 130; leucocytes, 27,000; polymorphoneuclears, 72 per cent. On August 16 she had a sudden effusion into the pericardium, and died.

Skiagraphs had been taken and showed breaking in the periosteum of the left femur, but nothing more.

At the post-mortem a large quantity of bloody serum was found in the pericardium. The empyema was practically well. On opening the left thigh an abscess was found, beginning opposite the point where the focus had been located clinically, separating the periosteum from the inner side of the femur, extending up to the under side of the joint where it perforated

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into the joint, and the joint was full of pus. The femur was split longitudinally, showing the epiphysis healthy, but the focus beginning in the medulla of the femur extending upward. Had this patient lived longer the epiphysis would undoubtedly have become separated, and this might have been pronounced a case of acute epiphysitis by a careless observer.

CASE 3.—Therese S., aged 12, was admitted to the University Hospital July 20, 1915.

Ten or twelve days before admission she complained of pain in the right hip and pelvis, with temperature from 100° to 104,° but could walk about. She was admitted with temperature of 100.9,° and pulse of 126; leucocytes, 22,000; polymorphoneuclears, 65 per cent. The patient had slight flexion of the right hip, and complained of some pain in the hip, thigh, and pelvis. There was slight swelling over the right hip-joint. The slightest motion in the joint caused intense pain. Pressure made directly over the joint from front and back elicited excruciating pain. There was no tenderness on pressure in the femur.

A diagnosis of acute osteomyelitis of the neck of the femur was made. The writer believed that this was a typical case with an early diagnosis and one well-fitted for the demonstration of the correctness of his views. So he requested one of the junior staff to operate upon it exactly as he would upon an acute osteomyelitis in any other location. Dr. Strachauer opened the hip-joint from the front, and found some serum in it, but it looked comparatively healthy. The neck of the bone was so soft that an instrument could readily be pushed into it. A small opening was drilled directly into the neck of the femur and a drainage tube introduced into this opening, and the capsule of the joint closed around it. There was no flow of pus following the opening into the bone, but from the subsequent history of the case no one can doubt that this was the original focus of infection. We all know that our most brilliant results from operations for osteomyelitis are in those cases in which the diagnosis and treatment have been so early that little or no pus has been formed, but in which the symptoms have been relieved by the operation.

The *following day her temperature dropped to normal,* and varied from then on between normal and 99.5,° disappearing entirely in the course of a few days. The drainage-

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tube was removed on the tenth day. On August 4 the wound was completely closed, and the joint was freely movable and free from pain. On August 16 she was up, walking on crutches. The pain was relieved immediately after the operation, and she was sitting up in a chair on July 30. She was first allowed to bear weight on the joint on August 19. She was finally restored to health with a healthy, normal hip-joint.

The infection in this case was not so virulent as in the average case, which accounts for the small amount of destruction that had taken place when the diagnosis was made, but can anyone familiar with the ravages of osteomyelitis doubt what the result would have been had the case gone on without proper treatment? The diagnosis should have been easier had the infection been more virulent.

Case 4.—This case illustrates the fact that inflammation of the neck of the femur may be secondary to osteomyelitis elsewhere, and that it may be subacute in character. A boy, aged 12 years, was admitted into the University Hospital with a severe osteomyelitis of the left tibia, which was operated upon promptly, after which he began to improve rapidly. After a few days he began to complain of discomfort in the right hip. There was no decided rise of temperature, and the pain was not severe. The symptoms were so slight that too little attention was given him until we noticed that the limb was everted and shortened. The joint was opened, and found full of pus. The epiphysis was separated, and the neck of the femur was necrotic, so that a formal resection of the hip was made, after which he made a speedy recovery.

It is only with an earnest desire to bring out the truth that this case is reported, for it is not very creditable to us. We should have made the diagnosis early, and drilled into the neck of the femur and saved the boy's hip. Our only excuse is the remarkable mildness of all the symptoms.

It is very evident that when Macnamara suggested the name acute "epiphysitis" he meant the term to apply to the whole neck of the femur, but the universal application of the term "epiphysis" to that portion of the head of the hip beyond the epiphyseal line has led to an unfortunate misunderstanding and much bad surgery. The term "epiphysitis" should therefore be

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abandoned. The name "osteomyelitis," as applied to pyogenic infection of the neck of the femur in this paper, is incorrect because the neck of the femur has no medulla, but is used by the writer to emphasize the fact that acute pyogenic infection of the neck of the femur in growing bone does occur, that it resembles osteomyelitis in every particular, and that it demands the same prompt treatment. It is even a greater menace to human life than osteomyelitis because it is more difficult to diagnosticate, and when the original seat of infection breaks down the opening is into the hip-joint, instead of the soft parts. These cases are not so rare but that they come under the observation of every surgeon, and they demand more careful consideration than they have received heretofore. They offer a field for brilliant, life-saving surgery. When the diagnosis is made very early, before the joint is involved, the operation originally suggested by Macnamara and later advocated by Huntington should be performed. An incision should be made over the great trochanter, and an opening drilled through the trochanter and the center of the neck of the femur in its long axis until the seat of infection is reached. When the diagnosis is not made until the joint is involved it is better to enter the joint directly from the front. The only reason the Macnamara operation was not performed in Case 3 is that the diagnosis was questioned by some members of the staff, and we wanted to see just what we were doing.

When the late operation is done a formal excision should not be made unless the neck is completely necrotic, but as much of the neck should be preserved as possible for future usefulness.

While the writer advocates the abandonment of the term "epiphysitis" he has no new name to offer. The term "acute suppurative arthritis" is objectionable,

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because there are many cases of acute suppurative synovitis in which the bone is not involved and they are not nearly so serious. Would it not be well to revive the old term "acute ostitis of growing bone"?

DISCUSSION

DR. ARNOLD SCHWYZER (St. Paul, Minn.): Unfortunately, I did not have a chance to read Dr. Moore's paper, but I see he has picked out of that large group of osteomyelitic infections one of the most interesting ones, the one located near the articular surface, especially of the knee-joint and hip-joint, which comes on especially in young children and adolescents. They are usually small foci in the beginning, usually cortical or periosteal. Sometimes they are quite small, and have a tendency to break through early into the joints; then they give us the picture of acute arthritis. The pus, instead of entering the joints, may find its way to the outside of the joint capsule, and cause a pararticular phlegmon.

It is the coxitis, the early involvement of the hip-joint, that Dr. Moore has brought before us especially. These small osteomyelitic foci develop, it seems to me, at the place where the bone has got to stand the most mechanical injury. Usually, it is at the femoral insertion of the iliofemoral ligament, or it is back of the crucial ligaments at the knee where we get a small focus and sequestrum.

Dr. Moore emphasized the importance of early recognition of these cases and of having them operated on as early as possible. If we have an osteomyelitic focus and remove it amply, we will often see the joint heal very surprisingly promptly.

One-half of these cases when seen in small children are due to the staphylococcus group. The other half is represented equally by streptococci and pneumococci. Staphylococci, because they form what their name designates, clusters of cocci, will easily block these very narrow young formations of capillaries at the metaphyseal line. This cluster-formation is probably the reason why the staphylococci are so predominant in osteomyelitis. The frequency of streptococci and pneumococci in these special cases of small children speaks for some mechanical cause; the smallest hemorrhage or injury at the insertion of the strong ligaments would produce a locus minoris resistentiæ. The ease and facility with which

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these joints heal when the pathology is recognized, was brought to my attention a short time ago in a case in which we had a focus of infection near the coronoid process of the ulna, which had existed for some time. The patient had had some pain in the elbow for a long time. Of late things became acutely worse,—some fever (101°), great swelling, and redness set in. We found a small focus with sequestrum in the ulna, and when we scooped it out a thick white pus came out of a small opening into the joint. Figuring it was an early case of joint-infection with a long previous chance for general immunization, and that we had only secondary involvement of the joint, we did not drain the joint further than by passing three strands of silkworm through the center of the bone cavity into the joint. Iodoform, boiled in bichloride of mercury, was rubbed into the bone-cavity, which was left to fill with blood. The wound was firmly closed with three strands of silkworm, which were removed after six days. There was no filling again of that joint; and it healed without the slightest difficulty.

Dr. Dean Lewis (Chicago, Ill.): I would like to ask Dr. Moore if he has had any case in which the focus of infection began in the acetabulum; and, if he has, in what way he would make an early differential diagnosis.

I recently had a case come to the hospital with the diagnosis of osteomyelitis of the upper end of the femur. I was not quite satisfied with that diagnosis, and after repeated explorations I found a rather putty appearance of the acetabulum, which did not satisfy me that we had osteomyelitis to deal with. While I was waiting for the diagnosis, it perforated the acetabulum, and an abscess formed under the iliac muscle. Undoubtedly, there was bone-infection, but drainage of the abscess resulted in a cure. There was definite bony formation about the neck of the femur.

Dr. Moore (closing): Answering the question of Dr. Lewis, I will say, that undoubtedly, in the case he speaks of, the infection began in the acetabulum, but the physician who first saw the case did not know it. I can imagine it would be difficult to make a differential diagnosis.

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ON THE CONDITION OF THE VASOCONSTRICTOR CENTER DURING THE DEVELOPMENT OF SHOCK

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So many reviews of the literature on surgical shock have appeared that we deem it unnecessary to enter here into such a discussion. The term is in itself too generic, and, although we make no attempt to define it, we use the term surgical shock to cover that compromise of the animal which results from operative trauma and which is analogous to the compromise that occasionally follows surgical therapeutic procedures on man. Our interest is centered in the view, which has been accepted too generally and upon insufficient evidence, that in surgical shock the *primary* cause of all the other symptoms is a preceding paralysis of the vasomotor center. Since Porter first attempted to show that the vasoconstrictor center is still in a state of pronounced functional activity after a shock level of blood-pressure has been reached, there have been attempts made by several investigators to bring forward evidence bearing upon this point.[1]

PROBLEM

Our problem was essentially this: Can we obtain any conclusive evidence that, during the development of shock, the vasoconstrictor center at any time loses its activity? If we could show that the center loses

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its activity very early, it would be reasonable to believe that some, if not many, of the other symptoms of shock, are secondary to this failure. If, on the other hand, indisputable evidence could be obtained that the activity of the vasoconstrictor center persists for a long time, and even after many of the other manifestations of shock are present, we could then say definitely that shock is not the result of a primary breakdown of the vasomotor center, but must be due to some other cause or causes.

Our first requirement was a region of blood-vessels the caliber of which could be observed without any operative interference; second, a region the observations upon which could be controlled by comparing it to its denervated mate upon the other side of the midline, in the same animal.

The retinæ of albino rabbits are non-pigmented, and the retinal vessels stand out distinctly. In our early experiments we tried to denervate the vessels of one retina, and, after producing shock in the animal, to observe and compare the size of the vessels in the two retinæ. Theoretically this method should be satisfactory, but for practical reasons it was finally discontinued. We need mention here only two of these reasons. First, the origin and course of the vasomotor supply to the retinal vessels is not perfectly understood; consequently, it might be claimed that the retinal vessels of the supposedly denervated side were not entirely isolated from connection with the vasomotor center, in which case we should have no control vessels. Second, it is necessary to depend upon the judgment of the ophthalmologist for a comparison of the vessels in the two eyes, thus introducing too many opportunities for error and furnishing us no chance to own results by direct observation.

ly selected the ear vessels of white rabbits.

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This region seems almost ideal for our purpose. First of all, the vessels can be seen distinctly by anybody. This means that a comparison of the ear vessels can be made by any number of individuals, either trained or untrained. Secondly, the vessels are subjected to no direct operative interference at any time. Thirdly, the course of the vasoconstrictor nerve fibers to the ear vessels has been well worked out,[2] and we can be sure therefore that the vessels of the control ear have really been disconnected from the medullary center.

The following is the plan by which we attacked the problem: If the vasoconstrictor nerves to one ear are cut we remove all influence of the vasoconstrictor center over the vessels of that ear. The denervated vessels might then be expected to react more or less passively to changes of pressure within them. If the heart were maintaining a high blood-pressure these vessels should become passively dilated. If, on the other hand, the blood-pressure were low they should be correspondingly less dilated. In other words, the control vessels of the denervated ear should react passively, no matter what the pressure within them. Why do we need such a set of control vessels? Suppose we reduce an animal to a state of shock, and find that ear vessels connected with the vasoconstrictor center are empty. This emptiness may be either an active or a passive one. That is, it may be caused by a stream of vasoconstrictor impulses flowing out from the center in the medulla and maintaining in the constricted vessels a tonus sufficiently active to force out the blood, or it may be due to a passive drainage of the blood from these vessels into the great splanchnic reservoir. If we denervate the vessels of one ear, the vessels of that ear—if they respond passively to pressure changes within them—will tell us whether the constriction present in the normal ear is an active or a passive one.

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Especially is this true if we can, by suitable means, raise temporarily the blood-pressure from its low shock level. The denervated vessels should passively dilate in response to this elevation of pressure. The normal ear vessels, if they are receiving no vasoconstrictor impulses, should also dilate; if, however, they resist the rise in pressure and remain constricted, we are justified in concluding that the constriction is the result of vasoconstrictor impulses arising outside the vessels, presumably in the medulla.

Furthermore, the ability of the vessels of the normal ear to remain constricted under the temporarily increased blood-pressure should be an index of the degree of tonus being maintained by the center in those vessels. For example, if the constriction persists in the normally innervated vessels after the blood-pressure has risen to a high point, we must conclude that the center is maintaining a high degree of tonus.

The question of the degree of passive response obtainable from denervated vessels at different intervals after denervation will be discussed a little later.

EXPERIMENTAL METHOD

As already stated, white rabbits were used exclusively. The vessels of one ear were denervated by resecting and severing, under ether anesthesia, the auricularis magnus nerve at the base of the pinna and removing the superior cervical sympathetic ganglion on the same side. In no case was the animal reduced to a state of shock immediately following the denervation of the ear. At least twenty hours elapsed between the two operations, and in some cases this interval was a number of days in length. The purpose of this delay was to avoid the effects upon the denervated vessels of immediate stimulation of vasodilator or vasoconstrictor nerve fibers consequent upon the

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operative procedures at the time of the denervation. After this interval, the animal was again etherized and reduced to a state of shock. In most cases it was not necessary, nor indeed advisable, to continue the etherization for more than three-quarters to one hour, for at that time most animals showed only a slight reaction, if any, to sensory stimulation. Tracheotomy was performed, and the abdomen opened wide by a long median incision. In a majority of the experiments the adrenals were carefully removed at once upon opening the abdomen. This was done on the assumption that adrenalin thrown into the blood-stream during the experiment might possibly interfere with a passive dilatation of the vessels of the denervated ear, and at the same time favor constriction in the vessels of the normal ear, a constriction which would be of peripheral rather than of central origin. Also it was thought that the removal of the adrenals might hasten somewhat the fall of the blood-pressure. We are unable to draw any definite conclusions as regards either of the above possibilities since we made no systematic comparison between animals with intact and with excised adrenals. At any rate, the removal of the adrenals, practiced in most of the experiments, with the attendant manipulation of and traction upon the viscera and sympathetic nervous structures may be considered one of the means used to induce shock.

Next the abdominal aorta was exposed for a short distance between the origin of the celiac artery and the diaphragm, and a loose ligature passed around it to facilitate the application of a clamp when desirable. Clamping the aorta at this point was the method used to raise temporarily the blood-pressure, in the part of the animal anterior to the diaphragm, when the general blood-pressure had fallen to a low level. The

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reason for raising the blood-pressure at this time has already been stated. Tracings of the blood-pressure were taken from a mercury manometer connected with a cannula in one axillary artery. As an index to the degree of shock we have relied largely upon the state of the blood-pressure; we did not confine ourselves, however, to observations upon this one symptom of shock. The state of the reflexes was used as was also the response to sensory stimulation applied to various regions. We continued the experiment, not only until the blood-pressure had fallen to a low level, but until the reflexes became sluggish or were practically absent, and there was little, if any, response to handling of the abdominal viscera or parietal peritoneum. The rabbit seems to sink to this condition much more readily than do most dogs.

As a second and final test of whether the constriction in the normally innervated vessels was an active or a passive one, we denervated, late in the experiment, the vessels of the normal ear. This was done in some cases by cutting the nerves, in others by abolishing the nerve conductivity through direct application of an ether soaked pledget of cotton to the nerve trunk, or by freezing the nerve at one point with an ethyl chloride spray. We found that, for practical purposes, severing the conductivity of the cervical sympathetic trunk was sufficient to bring out the effect in the "normal" ear vessels, and therefore in most cases we did not cut the auricularis magnus nerve. Another reason why we usually left the auricularis magnus intact was that we wished to disturb the animal as little as possible. (The cervical sympathetic is the more easily obtainable of the two nerves that carry a vasomotor supply to the ears.)

In a few experiments the ears of the animals were photographed during the course of the experiment.

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For this purpose we used light transmitted through the ears to the camera from an electric arc. In one case a water-color drawing was made from life toward the end of the experiment.

EXPERIMENTAL RESULTS

Twenty-five experiments were performed in this series. The interval between denervation of the blood-vessels of one ear and the production of shock varied. In eleven experiments this interval was 20 to 28 hours; in two experiments it was 2 days; in five experiments it was 3 days; in two experiments 4 days; in two experiments 6 days; and finally one experiment each was performed in which the interval was 7, 9, and 12 days, respectively. As has already been stated, this interval was allowed in order to secure a proper set of control vessels. It concerns in no way the vessels of the normal ear directly.

ON THE SUITABILITY OF "ONE-," "TWO-," "THREE-DAY" (AND MORE) RABBITS FOR THIS KIND OF TEST

The term "one-day," "two-day," or "three-day" rabbit will refer to the length of the interval which elapsed between the denervation of the blood-vessels of the control ear and the execution of the shock experiment.

We found early in the series of experiments that the denervated vessels of one-day rabbits responded best. That is, their response was most nearly a passive one. If a longer time elapsed the denervated vessels showed an increasing tendency, with the increase in the length of the interval, to constrict and a decreasing tendency to react passively to pressure changes within. The following figures will serve to make this point clear.

In eleven experiments we used one-day rabbits. The denervated vessels invariably showed good pas-

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sive responses to pressure changes. In five experiments we used three-day rabbits. In four of these the passive response of the denervated vessels was good, but in the fifth scarcely any passive response could be obtained. Two four-day rabbits were used. One of them failed to show a passive response. Of the two six-day rabbits used one failed to respond passively. Finally, one experiment each was performed upon seven-day, nine-day, and twelve-day rabbits respectively. The seven-day rabbit gave a good passive response, but the remaining two showed practically no passive reaction whatever. To summarize these results we may say that in every case when used within one day after denervation (11 cases), the passive reaction of the vessels was good; that they failed to react well in one out of five three-day rabbits, while in seven experiments upon rabbits of four days or over, the failure to respond passively was seen in four, that is, in about 57 per cent.

What is the significance of these results? It is this: We require, for our demonstration, vessels which respond passively to pressure changes within them in order that, by comparison, we may determine the state of tonus being maintained by the vasomotor center in the vessels with which it is still connected. The results stated above show that, to obtain this passive response in the control vessels, one-day rabbits are best; that, while it is sometimes possible to obtain a passive response, even as late as seven days after denervation, still in many cases the vessels have developed by that time too much autonomy to serve as passive controls.

The above observation agrees with statements in the literature[3] to the effect that the dilatation of the vessels of the denervated ear gradually decreases and finally may even disappear. The time necessary for

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the latter to develop is stated by Meltzer, however, to be a matter of weeks.

It is stated by Bayliss[4] that the arterial wall reacts to an increase in arterial pressure by constricting. If this statement be true, can we use the denervated ear vessels as passive controls? Anrep,[5] working later upon the same subject in Bayliss' laboratory, concluded that "a local form of reaction of the blood-vessel wall to changes of tension is as yet unproven." Even though Bayliss' original interpretation were correct, namely, that, if the pressure within an artery is increased, the artery responds by contracting, or, that, if the pressure falls the arterial wall relaxes, his results would not necessarily be in conflict with our findings. This is true for the following reasons:

Bayliss does not say to what degree the arteries became constricted, and there is certainly no evidence that they responded to the increased pressure with a strong constriction. It may indeed be true that the arteries and arterioles in the denervated ears of our animals were not as much dilated as they would have been had they not possessed any such reaction to internal pressure. And especially may this be true of the denervated vessels a number of days after the denervation, when they show a definitely greater tendency to remain constricted. That these vessels were more dilated in our experiments after raising the blood-pressure than they were before, however, cannot be questioned. In many cases the dilatation was easily visible across the entire width of the room, and seemed to be practically maximal.

Again, the constriction described by Bayliss was apparently of short duration. He does not give, in all cases, the full tracing, but in one that he does give (Fig. 7, p. 226) the duration of the constriction was about one minute. In some other cases it was ap-

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parently somewhat longer. In our experiments the high blood-pressure was usually maintained for from three to five minutes, and sometimes (see protocols below) even longer. A transient tendency to constrict would, therefore, have disappeared in all probability before we removed the aortic clamp. Moreover, in our experiments, the increase in blood-pressure developed slowly, and not suddenly, as was the case in Bayliss' experiments, for the heart does not build up a high blood-pressure in the head region instantly upon clamping the aorta. Since usually the efficiency of a stimulating agent varies directly with the suddenness of its application, perhaps the tendency of the vessels to react when the increase in internal pressure is developed slowly is less than when it is rapid.

When all these factors are considered it can be seen that there is not necessarily a conflict between the results obtained by Bayliss and those here described. Perhaps the most conclusive evidence that the denervated vessels do dilate when subjected to a high blood-pressure is furnished by the well-known fact that after cutting the cervical sympathetic nerve in the rabbit (as in the Claude Bernard experiment) the ear vessels on the corresponding side become widely dilated. This result obtains in an otherwise normal animal and therefore in one with a high blood-pressure. It must be admitted, therefore, that at least to the extent to which these vessels dilate under high pressure, and empty themselves when the pressure falls, they may be used as passive controls for the blood-vessels still connected with the center in the medulla.

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Fig. 1. To show the contrast between the constricted vessels still connected with the vasoconstrictor center (left ear) and the control dilated vessels that have been disconnected from that center (right ear). Drawing made late in the experiment when the animal was apparently in a state of deep shock. The strong vasoconstriction in the left ear was replaced by a wide dilatation as soon as the connection of this ear with the vasomotor center was severed. (See Protocol V.)

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Fig. 2. The rabbit is in shock. The left ear (constricted vessels) is in connection with vasoconstrictor center. The right ear (dilated vessels) has been cut off from central control. (See Protocol V.)

Fig. 3. Same rabbit as shown in Fig. 2. Photograph taken later in the experiment after the vessels of the left ear had also been cut off from central control. The formerly well-contracted vessels of the left ear are now well dilated. (See Protocol V.)

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RESULTS OF THE SHOCK EXPERIMENTS

There was a remarkable constancy in the behavior of the vessels of the normally innervated ear. The following protocols are typical of the results obtained:

PROTOCOL I

Experiment 14.—May 15, 1914. One-day Rabbit. White male; 1,700 gm. Yesterday at 4:00 P. M. denervated the right ear.

3:00 P. M.	Began operation. Before operation vessels of normal ear well constricted; those of operated ear fairly well dilated. After tying down the animal, vessels of both ears dilated.
3:56	Operation completed. It consisted of tracheotomy, wide opening of abdomen, removal of adrenals, and freeing of aorta just below diaphragm. Canula inserted in left axillary artery for blood-pressure record.
4:00	Blood pressure 33 mm. Hg. Lid reflex fair. Denervated ear moderately dilated. Normal ear vessels slightly smaller.
4:09½	Clamped aorta. Blood-pressure began rising rapidly.
4:11	Vessels denervated ear widely dilated. Normal ear moderately constricted.
4:13	Blood-pressure 94 mm. Hg. Lid reflex active.
4:14	Clamp off aorta.
4:15	Denervated ear still moderately dilated. Normal ear pale; vessels strongly constricted. Blood-pressure 30 mm. Hg.
4:25	Aorta clamped again. Blood-pressure rapidly rose to 87 mm. Hg.
4:26	Denervated ear widely dilated. Normal ear blanched; bloodless.
4:35-40	Cut the auricularis magnus, and removed superior cervical ganglion on left side—that is, for normal ear. "Normal" ear vessels became maximally dilated. Aorta is still clamped, and blood-pressure high.

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4:45	Clamp removed from aorta. Blood-pressure fell at once to 22 mm. Hg.
4:47	Vessels both ears same size (moderately dilated).
4:48	No lid reflex. Blood-pressure 22 mm. Hg.
4:49	Clamped aorta.
4:52	Blood-pressure 88 mm. Hg. Slight lid reflex. Vessels both ears practically maximally dilated. Clamp off aorta.
4:57	Blood-pressure 36 mm. Hg. Slight lid reflex. Vessels of both ears moderately dilated.
4:57½	Clamped aorta again.
4:59	Blood-pressure 100 mm. Hg. Vessels both ears maximally dilated.
5:00	Clamp off aorta.
5:08	Animal killed.

In this experiment the blood-pressure had fallen within one hour after the beginning of the operation to 32 mm. Hg. pressure, that is, to about one-third of the pressure normally present. At this pressure there was only a slight, though definite, difference between the vessels of the two ears. When the aorta was clamped at 4:13 and the blood-pressure in the head region rose quickly to 94 mm. Hg., a marked contrast appeared between the vessels of the two ears. The denervated vessels became widely distended with blood, while the normally innervated vessels continued to be moderately constricted. When the aorta was unclamped the blood-pressure fell quickly to a low level again, namely, to 30 mm. Hg. There was still a difference, though not a marked one, between the ears. At 4:25 the aorta was clamped, and the blood-pressure rose to 87 mm. Hg. This time the difference between the two ears was even more marked than after the previous clamping, for the protocol reads that this time the normal ear was "blanched and bloodless," whereas before it was described as being "moderately

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constricted." In other words, it might be thought that the vasomotor center had not only not lost any of its grip upon the blood-vessels during the past fifteen minutes, but had actually increased its tonus. At this point in the experiment the final step in the test was made to determine whether the vessels of the normal ear were constricted because of the activity of the vasomotor center,—that is, the vasomotor nerves of the normal ear were quickly exposed and severed. The result was unmistakable, for the "normal" vessels quickly became maximally dilated. At no time thereafter were they again smaller than those that had been denervated the day before. Twice, later, the aorta was clamped and unclamped. The blood-pressure rose from 22 to 88 mm. Hg. and from 36 to 100 mm. Hg., respectively. Each time the vessels of both ears dilated strongly with the rise in blood-pressure, and emptied somewhat when the fall came,—that is, they both reacted in the *same* direction, in strong contrast to their reaction before the "normal" ear was denervated.

The following is an example of a one-day (twenty hours) rabbit in which, toward the end of the experiment, the cervical sympathetic was frozen, instead of cut, as in the former experiment:

PROTOCOL II

Experiment 32.—July 30, 1914. White female rabbit; 1,550 gm. Vessels of right ear denervated 20 hours before shock experiment.

8:10 A. M. Before disturbing animal in cage the vessels of normal ear were strongly constricted, while the denervated ear vessels were widely dilated.

8:15 Began operation. Method exactly same as in Experiment 14 (Protocol I), except that this time adrenals were not removed.

8:50 Operation completed. Vessels of both ears same size; moderately dilated.

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9:06 Have been handling abdominal viscera for over ten minutes. Normal ear well constricted; denervated ear well dilated. A marked difference between them. Lid reflex good. Respiration 132.

9:43 Blood-pressure 46 mm. Hg. Normal ear well constricted. Denervated ear moderately dilated.

9:44 Clamped aorta.

9:46 Blood-pressure 72 mm. Hg. Normal ear still well constricted. Denervated ear vessels gorged with blood.

10:15 Clamp off aorta.

10:25 Again clamped aorta.

10:28 Normal ear vessels well constricted. Denervated vessels widely dilated.

10:30 Froze left cervical sympathetic nerve with ethyl chloride spray. Vessels of "normal" ear became quickly as widely dilated as those of denervated ear.

10:45 Clamp off aorta.

11:08 Blood-pressure 30 mm. Hg. Respiration 44 per minute. Lid reflex sluggish. "Normal" vessels slightly dilated. Denervated vessels moderately dilated,—that is, somewhat wider than "normal" vessels.

11:09 Clamped aorta again. Blood-pressure rose to 54 mm. Hg. Denervated ear vessels gorged. "Normal" ear (nerve frozen at 10:30) well dilated, but definitely less so than denervated ear vessels.

11:10½ Clamp off aorta. Blood-pressure fell at once to 23 mm. Hg. Lid reflex sluggish. Animal seems deep in shock. Allowed to die.

The results of this experiment are essentially the same as those of the previous one. The normal vessels remained constricted until the conductivity of the cervical sympathetic nerve on that side was temporarily abolished by freezing it with the ethyl chloride

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spray. They became then as widely dilated as the denervated vessels. The freezing was discontinued as soon as the vessels of the corresponding ear dilated. That the conductivity of the nerve had not been permanently abolished by the freezing is shown by the fact that 39 minutes later (11:09) when the aorta was again clamped the normal vessels did not dilate so strongly as the denervated vessels. Some permanent injury had evidently been done, however, for the normal vessels did not return again to a state of strong constriction. It will be noted that a good dilatation was obtained in this case by destroying the conductivity of only one of the nerves supplying the vessels of the normal ear.

In the following experiment the control ear was denervated three days before the shock experiment.

PROTOCOL III

Experiment 34.—Sept. 12, 1914. White male rabbit; 1,250 gm. Left ear denervated three days ago.

8:10 A. M.	Vessels of normal ear constricted. Those of denervated ear well dilated.
8:20	Operation began—tracheotomy, abdomen opened wide, aorta exposed just below diaphragm, canula in left axillary artery for blood-pressure. Adrenals left intact.
9:33	Operation completed. Blood-pressure 88 mm. Hg.
9:40	Lid reflex sluggish. Respiration 48, slow, efficient, regular. Normal ear vessels strongly constricted. Denervated vesesls slightly dilated.
9:50	Clamped aorta.
9:54	Normal ear as before strongly constricted. Denervated vessels well dilated. A marked difference between them.
9:57	Clamp off aorta.
10:32	Blood-pressure 53 mm. Hg.

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10:32½ Clamped aorta. Blood-pressure rose rapidly to 108 mm. Hg. Normal ear vessels bloodless as before. Denervated vessels gorged with blood. The difference can be seen across the room without difficulty.

10:37 Clamp removed from aorta. Blood-pressure fell to 49 mm. Hg. Heart became irregular and slow. Lid reflex gone. Struggling, apparently dying. Clamp aorta again. Blood-pressure rapidly rose to 119 mm. Hg.

10:42 Blood-pressure 120 mm. Hg. Normal ear absolutely bloodless and blanched. Denervated vessels gorged with blood.

10:50 Clamp off aorta. Blood-pressure fell to 48 mm. Hg.

10:52 Blood-pressure very low. Lid reflex sluggish. Respiration 40 per minute, regular, efficient. Normal ear vessels blanched. Denervated ear pink, but vessels slightly constricted. No response to handling of viscera or stroking of parietal peritoneum. Aorta clamped.

10:52½ Ether applied to trunk of right sympathetic.

10:53½ "Normal" ear vessels widely dilated. Somewhat wider, in fact, than vessels of ear denervated three days ago.

10:55 Ether washed off nerve. Clamp off aorta.

11:05 Denervated vessels slightly dilated. "Normal" vessels somewhat smaller than control vessels. Aorta clamped.

11:08 "Normal" ear vessels definitely smaller than those of control ear, which are well dilated. Again applied ether to cervical sympathetic, and normal vessels dilated as before.

Experiment discontinued.

The first point to be noted in this experiment is that the control vessels reacted very satisfactorily to internal pressure changes. If they resisted at all the tendency toward dilatation when the blood-pressure was raised by clamping the aorta, it was certainly not to any marked degree, for the word "gorged" de-

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scribed best the picture presented at that time. This engorgement did not confine itself to the main trunk vessels of the control ear. The whole ear was suffused to a marked extent. Inasmuch as three days elapsed between the denervation of this ear and the execution of the shock experiment, we believe it can hardly be claimed that this dilatation was in any sense due to a persistent stimulation of vasodilator nerve fibers that were injured three days before when the nerves were cut. The evidence in favor of this view is even more conclusive in the protocol which follows, for in that case *seven* days elapsed after cutting the nerves of the control ear before the shock experiment was carried out, yet the conduct of the vessels of the control ear was essentially the same as in the experiment under discussion.

The conduct of the vessels of the normal ear in this experiment is striking. Two and a half hours after the beginning of the experiment the blood-pressure had fallen to 48-49 mm. Hg. (see Protocol at 10:37 and 10:50). When the aorta was clamped at 10:42 the blood-pressure rose to 120 mm. Hg., that is, to a high level for the normal rabbit. The general condition of the animal, at this stage of the experiment, was bad. The lid reflex was sluggish, and the sensory responses were very poor or entirely absent. In spite of the state of the animal, generally, and in spite of the very high blood-pressure, the normal ear (10:42) remained "blanched and bloodless." Furthermore, the vessels remained persistently in a state of what seemed to be maximal constriction until the conductivity of the cervical sympathetic nerve on that side was abolished by the application of ether. Immediately thereafter the vasoconstriction of the normal ear was replaced by a marked dilatation, thus furnishing evidence that the preceding constriction was due to

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the active influence of the vasoconstrictor center. We wish to call attention to the following additional evidence that we are dealing here with an active vasomotor center. The ether was washed off the cervical sympathetic as quickly as possible after the vessels of the "normal" ear dilated. This was done to prevent, if possible, a permanent abolition of the nerve conductivity. We had the satisfaction of seeing later a partial recovery of tonus by the vessels of the "normal" ear,—that is, these vessels again became distinctly smaller than those of the control ear under the same pressure. That they did not again become strongly constricted, was due in all probability to some permanent injury produced by the ether. A second application of ether to the cervical sympathetic again brought about a strong dilatation of the "normal" vessels.

The fact that the vessels of the *normal* ear became slightly more dilated, after application of the ether to the cervical sympathetic, than those of the control (denervated) ear, illustrates the statement made above, that vessels several days after denervation usually show a greater tendency to resist the dilating influence of internal pressure than do vessels that have just been denervated.

PROTOCOL IV

Experiment 17.—June 16, 1914. Seven-day rabbit. White male. 1,850 gm. Right ear denervated seven days before shock experiment.

8:14 A. M. Vessels of denervated ear definitely, but only slightly; wider than those of normal ear.

8:15 Began operation. Tracheotomy. Abdomen opened wide, adrenals removed; canula in abdominal aorta at level of bifurcation into iliacs, for blood-pressure tracing.

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9:08	Began registering blood-pressure. Blood-pressure 42 mm. Hg. Normal ear vessels wide. Denervated ear vessels small.
9:25	Normal ear blanched. Denervated ear moderately constricted, but vessels definitely wider than normal ear vessels.
9:33	Clamped abdominal aorta above celiac. Normal ear remained blanched and bloodless. Denervated ear vessels fairly widely dilated, and the whole ear bright pink in color. Could not register blood-pressure while aorta was clamped.
9:38	Clamp off aorta.
9:39	Animal in very deep shock.
9:43	Blood-pressure 30 mm. Hg. Lid reflex very sluggish. Normal ear bloodless. Denervated ear well suffused. Vessels moderately dilated.
9:47	Abdominal aorta clamped above celiac again.
9:49	Normal ear blanched as before. Denervated ear vessels almost maximally dilated. The difference between the ears is very striking.
9:54	Clamp off aorta.
10:05	Blood-pressure 24 mm. Hg. Lid reflex very sluggish. Respiration regular, shallow, 52 per minute. Normal ear bloodless as before. Denervated ear moderately dilated and suffused.
10:10	Removed left superior cervical ganglion and clamped aorta above celiac.
10:12	Normal ear vessels (just denervated) almost maximally dilated. Wider than the other ear. Denervated ear vessels (former denervation) moderately well dilated.
10:28	Ears same as 10:12. Respiration 24 per minute. Lid reflex sluggish. Heart feeble and slow.
10:30	Clamp off aorta. Struggling. Blood-pressure 15 mm. Hg.
10:34	Animal nearly dead. Blood-pressure falling gradually. Killed.

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As regards the conduct of the vessels of the normal ear the experiment differed in no essential respect from the experiments already described. Before, during, and immediately after the operation, these vessels showed evidences of the usual varying intensity of vasoconstrictor tonus (8:14—moderate constriction; 9:08—wide dilatation; 9:25—blanched); later in the experiment, when the blood-pressure became very low, they showed no relaxation at any time. Whether the aorta was clamped or not, the normal ear vessels remained practically maximally constricted.

This contrast between the vasomotor reactions, obtained in the early and in the late stages of the experiment, was seen so frequently that we may say it was practically a constant phenomenon. The vessels of the normal ear were usually in a state of moderate constriction before the animal was disturbed in the cage, and practically always became widely dilated in consequence of the handling, tying down and operating,—in short, before the experiment began and during its early stages they were subject to varying degrees of contraction and dilatation. In the late stages, however, there was almost never a variation from a state of vasoconstrictor tonus sufficient to keep the normally innervated vessels strongly contracted. This may be interpreted to mean that early in the experiment, when the general blood-pressure control by the vasomotor center is ample, either a vasoconstrictor or a vasodilator response may be made by the center to a change in the environment; that later in the experiment, however, when the blood-pressure has shown a strong tendency to fall, the vasoconstrictor center marshals all its forces, and only one type of response is seen, namely a strong constriction; and that, moreover, this constrictor influence is maintained through a

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period when other functions show marked signs of giving way.

Again, in this experiment, we see that the "normal" vessels became strongly dilated as soon as the cervical sympathetic path was interfered with. The chief reason for presenting this protocol is that the control vessels, seven days after denervation, reacted in the same sense as did those of the one-day rabbits. It is surely reasonable to conclude that the dilatation seen in this case in the denervated ear, after clamping the aorta, was due to mechanical distention of the walls caused by the pressure of the fluid within, and not to any vasodilator influence of nervous origin. We regard this as a point of vital importance. If it be proven that the denervated vessels respond to increased pressure by a distention, then the normal vessels should, if left to themselves, react in the same way. If they react in an opposite sense it must be due to some outside influence. We believe we have shown by the experiments here described that there is an outside influence and that it can be removed by severing the connection between the vasoconstrictor center and the vessels. Moreover, we believe the foregoing evidence warrants us in concluding that the vasoconstrictor center is not the first of the vital centers to fail; in fact, there is some warrant for believing that at a time when other functions have yielded definitely, the vasoconstrictor center still shows marked activity.

Again in this protocol we find an illustration of a statement made in the early part of the paper, namely, that, if several days elapse after denervation, the denervated vessels show a considerable tendency to establish autonomy. This is evident throughout the experiment.

There is one other fact to be noted in this protocol which was noted also in a number of other animals

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experimented upon several days after denervation of the ear. The denervated vessels showed a greater tendency to contract at the beginning of the experiment than during the latter part. We do not wish to enter at this time upon an exhaustive discussion of the possible reasons for this apparent paradox. It might be said in line with Bayliss' experiments, that in the beginning the stronger tendency to constrict was the impulse furnished the muscular wall of the blood-vessel by the high blood-pressure; and that as the blood-pressure fell, during the course of development of shock, this tendency also diminished and the vessels dilated. Against this view stands the fact observed in several instances that the vessels dilated late in the experiment more than in the beginning, even if the blood-pressure, by clamping the aorta, was raised to from 70 to 90 per cent of the normal level. If the vessels were stimulated to constriction by the high blood-pressure in the beginning, they ought to respond to pressure in the same way later on. Since we plan to investigate further the pressure reactions of blood-vessels, we will leave a further discussion of this point until a future time.

The protocols already presented are representative of the results obtained in by far the majority of the experiments. We may designate as the type experiment one in which the development of shock was a gradually progressive phenomenon that could be represented roughly by a straight line with one end placed lower than the other. In some cases the descent into a condition of shock was relatively rapid; in others, very slow. The former of these could be represented by a line indicating a steep slope; the latter, by a longer line, having a much more gradual slope downward. In both of these the vasomotor center was strongly resistant. So far as one could judge from

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the vessels of the normal ear there was no time during the experiment, unless it were at the very end, that is, just before death, that the vasoconstrictor center was not active (we are not speaking of degree of activity now).

We have three experiments which present a variation from the above type and which throw an interesting light upon this struggle of the vasomotor center for control of a desperate situation. We may describe this type as one in which the slope, representing the general condition of the animal, is interrupted at one point by a sudden sharp descent of the line, followed by a more or less rapid return of the line to the level of the original slope. Such an experiment is the following:

PROTOCOL V

Experiment 28.—July 15, 1914. Male white rabbit; 1,550 gm. Shock experiment started twenty-four hours after denervating vessels of right ear.

8:05 A. M. Vessels of denervated ear well dilated. Those of normal ear well constricted.

8:10 Operation began. Method exactly same as in Experiment 34 (Protocol III).

8:50 Operation finished. Lid reflex good. Respiration regular and efficient.

9:03 Blood-pressure 34 mm. Hg. Vessels of denervated ear well dilated. Normal vessels well constricted.

9:15 Considerable blood lost accidentally from axillary artery. Struggling. Heart and respiration stopped. Lid reflex gone. Clamped aorta above celiac. Started artificial respiration and cardiac massage.

9:19 Recovering well now.

9:25 Just before animal nearly died, at 9:15, the normal ear was practically blanched, while the denervated vessels were well dilated. Then at

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9:19, with the recovery of the animal, the vessels of the normal ear became widely dilated (no vasoconstrictor tonus evident at all). Now the normal vessels are already beginning to constrict. They are definitely smaller than the denervated vessels again.

9:32 Vessels of denervated ear well dilated. Normal vessels practically blanched. Aorta still clamped. Blood-pressure 92 mm. Hg.

9:40 Took photographs of ears as at 9:32 (Fig. 2). While taking the photograph, observed that the vessels of normal ear showed excellent rhythmic contractions and dilatations at the rate of 6 per minute. The vessels oscillated between "blanched" and "slightly constricted." The rhythmic changes easily seen at a distance of three feet. Denervated vessels never showed rhythmic contractions.

10:10 Must leave clamp on aorta, otherwise blood-pressure quickly falls to near zero and heart stops. Cut left cervical sympathetic and removed ganglion.

10:20 Vessels of left (normal) ear already wide. More photographs (Fig. 3).

10:32 Blood-pressure 96 mm. Hg. with aorta clamped.

10:37 Clamp off aorta.

10:38 Blood-pressure 12 mm. Hg. Heart stops if clamp is removed from aorta for any length of time. Lid reflex nearly gone.

10:38½ Heart nearly stopped. Clamp on aorta again.

10:45 Lid reflex good. Respiration very slow, regular, and apparently efficient. Blood-pressure has risen to 35 mm. Hg. Vessels of left ("normal") ear (denervated at 10:10) slightly wider even than "denervated" vessels. Heart stops quickly whenever clamp is removed from aorta. Experiment discontinued.

Here we see an animal in which (until 9:15) the course of the descent into a state of shock was in no essential particular different from the usual one, which

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has already been described. At 9:15 an accidental hemorrhage nearly resulted in the death of the animal. Both the heart and respiration stopped, and the lid reflex disappeared. Within four minutes, however, through the use of artificial respiration and cardiac massage, the animal had revived, and was doing fairly well. The blood-vessels of the normal ear, which before the hemorrhage were strongly constricted, showed now a marked contrast to that condition. They were widely dilated, which means that there was little if any evidence of vasoconstrictor tonus. From this time on, however, the vessels gradually became smaller and smaller. At the end of six minutes they were already definitely smaller than those of the control (denervated) ear. At the end of thirteen minutes they were again as strongly constricted as before the hemorrhage. One of the photographs taken shortly after this (Fig. 2) shows something of the contrast between the vessels of the two ears, though it is by no means as striking as the actual picture presented by the ears themselves. The reason for this is twofold. First, the smaller vessels of the denervated ear that were visible to the naked eye do not show up distinctly, and the opacity of the larger vessels along their margins seems to have been insufficient to cast a distinct shadow upon the photographic plate. As a result these (larger) vessels seem narrower in the photograph than they actually appeared to the eye. Second, the photograph fails entirely to show the color differences of the two ears. The engorgement of the denervated ear was not confined to the larger vessels, but seemed to involve also those that were invisible to the eye for the whole ground substance of this ear had a definite and marked pinkish flush. The normal ear, in contrast to this, had a cadaveric whiteness, which was tinged with a suggestion of yellow rather than of

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pink. In addition to this blanching of the ear substance, the smaller vessels were invisible and the larger ones reduced to mere threads.

In order to convey a more accurate impression of this contrast, we had the artist make a water color sketch from life (see Fig. 1*). While it is of necessity partially schematic so far as the vessels of the ears are concerned, still it gives a much more exact general impression than does the photograph.

Figs. 1 and 2 represent the appearance of the ears at a time when the general blood-pressure was so low that the heart stopped when the clamp was removed from the aorta, thus allowing the splanchnic area and the posterior part of the body to be included in the circulation. With the aorta clamped just below the diaphragm, however, the heart built up a pressure in the head region equal to 92 mm. Hg., and in the face of this pressure the vessels of the normal ear remained constricted as shown in the two figures just mentioned.

A little later in the experiment the vessels of the normal ear were also denervated, whereupon the constriction of these vessels was quickly replaced by a wide dilatation. Fig. 3 is one of the photographs taken at this stage of the experiment. While it fails to bring out the flushing of the ear substance that developed, it does show fairly well the enlarged blood-vessels.

The description of the contrast between the normal and the denervated vessels just given holds in a general way for all the experiments in the series. The individual variations that were seen were variations in the degree of the contrast. When one-day rabbits were used, and the aorta clamped temporarily late in

* The water color sketch is not reproduced in the illustration.

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the experiment, the above described differences obtained in by far the majority of the experiments.

As already stated, Protocol V is one of a group of three experiments in which there was evidence of early vasoconstrictor failure. Of the two remaining experiments, which belong to this group, one was in all respects similar to Protocol V. That is, there was only a temporary loss of vasoconstrictor activity after stoppage of the heart, and this was followed by a good recovery. In the third experiment, although the animal recovered sufficiently to breathe without artificial respiration, it was not possible at any time thereafter to remove the clamp from the aorta without causing stoppage of the heart. This was apparently due to the fact that the venous return to the heart was insufficient, for, upon removing the aortic clamp, the arterial pressure quickly fell to a very low level, for example, from 62 to 14 mm. Hg., and soon thereafter the heart would slow up and stop. Moreover, during the remainder of the experiment the vessels of the normally innervated ear reacted to internal pressure changes in exactly the same way as those of the denervated (control) ear. All of which means that after the temporary stoppage of the heart and respiration early in the experiment the vasoconstrictor center never gave any evidence of recovery.

We have made no systematic study of the effects of hemorrhage or temporary cardiac failure upon the resistance of the vasoconstrictor center; but the results obtained in these three experiments indicate that, if such accidents complicate the situation, the behavior of the center is markedly different from what it is in those cases in which shock is produced by operative trauma alone.

We appreciate well the fact that in such studies as this it may always be disputed that the condition

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studied was really that of surgical shock. We wish, however, to call attention to this important point in connection with our experiments. *It is not essential that our animals be reduced to a condition which will be acknowledged by everyone to be identical with deep surgical shock as usually encountered.* We were convinced that our animals sank finally into such a state. It must be recalled, however, that our problem was to determine whether during the *course of development of shock* the loss of vasoconstrictor tonus is the primary cause of all the remaining symptoms, or whether this tonus persists until other symptoms have become marked. Whether, in fact, the activity of the vasoconstrictor center may not be well up to normal at a time when the general blood-pressure has fallen to a low level, and the other symptoms become strongly developed. This being the case, it was only essential that our animals be well on the way toward a condition of deep shock, and it can hardly be disputed that such was the case.

The method used in these experiments to demonstrate the presence or absence of tonus in the vasoconstrictor center could, we believe, be used as a means of demonstrating vasoconstrictor activity in other lines of experimentation just as well as in shock.

Furthermore, since we have, within certain limits, means for raising the blood-pressure at will, the *degree* of activity of this center could be determined by raising the blood-pressure until the normally innervated vessels begin to yield, that is, dilate, under the pressure. We might term this the "breaking-point." It might even be possible, by making a series of such determinations during the course of a single experiment, to plot a curve that would represent the behavior, as regards degree of activity, of the vasoconstrictor center.

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It has been stated[6] that the left ear of the rabbit is better supplied with vasomotor nerves than is the right. If this be the case, it would be better in experiments such as these to denervate the already poorly innervated right ear and use it as the control. In nearly all our experiments the right ear was used as the control.

SUMMARY AND CONCLUSIONS

1. Denervated blood-vessels usually show the best passive response to internal pressure changes when used within twenty-four hours after their denervation.

2. Normally innervated ear vessels are strongly constricted while the animal is sinking into a condition of shock and after shock has developed.

3. This strong vascular constriction persists even though the blood-pressure be raised well toward the normal level (at a time when the animal shows distinct symptoms of shock).

4. This vasoconstriction is due to the activity of the vasoconstrictor center because (a) the control denervated vessels become strongly dilated under the influence of the same pressure and (b) the constriction of the normal vessels themselves disappears at once if their connection with the vasoconstrictor center is destroyed by cutting the nerves, or by abolishing their conductivity with ether or by freezing.

5. Since a fairly high degree of activity of the vasoconstrictor center can be demonstrated even after the blood-pressure has fallen to a low level and reflexes are sluggish we believe it is justifiable to conclude that a paralysis or failure of the vasomotor center is not the *primary* cause of the other symptoms of surgical shock.

REFERENCES

1. Porter, W. T., and Quinby, W. C.: Am. Jour. Physiol., 1914, 10, xii; Porter, Marks and Swift: Am. Jour. Physiol.,

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1907, xx, 444; Porter and Quinby: Am. Jour. Physiol., 1908, xx, 500; Seelig and Lyon: Jour. Am. Med. Assn., 1909, lii, 45; Bartlett: Jour. Exper. Med., 1912, 15, 415; Mann: Bull. Johns Hopkins Hosp., 1914, xxv, 205.

2. Meltzer and Meltzer: Am. Jour. Physiol., 1903, ix, 57.
3. Meltzer and Meltzer: Am. Jour. Physiol., 1903, ix, pp. 62 and 159.
4. Jour. Physiol., 1902, 28, 220.
5. Jour. Physiol., 1912, 45, 318.
6. Meltzer and Meltzer: Am. Jour. Physiol., 1903, ix, 66.

DISCUSSION

DR. ARTHUR E. HERTZLER (Kansas City, Mo.): I want to touch on a somewhat different phase of the subject.

Here is the peritoneum (indicating on the blackboard); and here is the transversalis muscle; here is the rectus; and there is the mid-body line. At this point (indicating) the peritoneum is innervated in such a fashion. Here is the point I want to make particularly: take medullated fibers; they retain their sheaths until they get near the region of the peritoneum, and then they lose them. The terminal portion of the medullated nerves look like the sympathetics, and that is my way of telling them apart. The question is one of innervation of the viscera. I have dissected out the rami communicantes in every way suggested to me, and there is no way of demonstrating the medullated fibers, speaking practically.

Let us go back to the operations we were speaking about this morning. Supposing here is the gall-bladder and this the common duct (indicating), the nerves run down this way and come out of the semilunar ganglion. There is the ganglion (indicating). From here the nerves go out in many different directions, and here we have the rami communicantes. Here is the abdominal wall. We squirt anesthetic in here; make an incision and go on down here (indicating), and you have shock. The problem is, how are we going to prevent the afferent impulse over there? So far no light has been shed, but the proposition has been made to block the rami communicantes. This is all very well theoretically, but difficult to carry out in practice, and particularly in fat individuals, the type who usually carry gall-stones. That represents the problem. There is pain or bellyache produced from the presence of gall-stones. If there are no medullated fibers in that region the sympathetic must carry a painful impulse. If the cerebrospinal system is represented it must be by fibers that carry no sheaths. The practical problem is how to prevent

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impulses from reaching the central system. So far no practical means has been suggested for reaching this end.

Dr. J. W. Andrews (Mankato, Minn.): While Crile's treatment of shock has been attacked, I have not heard anything said about his method of preventing shock.

Dr. Frank G. Nifong (Columbia, Mo.): It may be presumptuous in me to discuss the question of shock when these men have been delving into it for nine years, but a pertinent question occurs to me in discussing Crile's work and his kinetic theory in the explanation of shock. We have as a clinical entity the so-called late or secondary shock, as you have all seen, and it does not differ in appearance from primary shock, which comes immediately after the operation or injury. For instance, I operated on a negro patient with pus tubes, and she has had them for months with a number of acute attacks until she is finally brought to me after a three weeks' illness with a temperature of 99,° nausea and vomiting, almost complete obstruction of bowels, and a leucocyte count of 18,000. I operate on this patient, and find a pelvis full of a firm inflammatory mass highly organized and adhesions, and the work was difficult because the lines of adhesion were not apparent. After much adhesion work and the removal of both tubes and an accompanying inflammatory mass, I find in the left pelvis a small abscess which I drain with a large Miculicz drain surrounded with rubber. She comes off the table in good condition, and remains so for about six hours, when rather suddenly she presents the clinical picture of profound shock and remains six or eight hours before rallying. Now, clinically, this is shock to be anticipated, if possible, and dealt with according to our best light. Now, what is the cause of this kind of shock? If emotions, if trauma, and if anesthesia make an impress on the central nerve centers and thereby cause shock, may not an overwhelming dose of protein poison, suddenly turned loose, likewise impress these centers and cause shock? In this case every day she received her maximum dose of poison. After operation a large absorptive area is made, and there is suddenly liberated an overwhelming dose with the resultant picture of shock.

Clinically, we must all recognize this as a picture of shock. We not infrequently get it unexpectedly when we disturb a septic focus, such as we see sometimes after curetting a septic uterus. Does it fit best the vasomotor theory of shock or

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some other? If some theory will help us to anticipate this, and also to deal with it successfully, we will welcome it.

It is not for me to discuss the confused and seemingly inexplicable subject of shock, but I present this phase of the subject to which you must make your theory accord.

Dr. M. L. Harris (Chicago, Ill.): The experiments of Dr. Seelig are very ingenious, but the great trouble in the study of shock has been that the different investigators have worked along a particular line and studied some particular symptom, or the influence of some particular agent, and have described that condition as shock. There has been a great deal of confusion as to what constitutes shock. I think we will admit that some of those who discussed the paper this morning, did not have a clear conception of what constitutes shock. We have shock and collapse and syncope,—conditions that are different, yet which are generally classified as one, namely, shock. Syncope and collapse and shock are not at all alike. They are very distinct. Viewing it from a clinical standpoint, let us see the difference. A person who goes into syncope may do so suddenly. There is a sudden inhibition of the heart's action, insufficient blood is forced to the brain. The patient drops in syncope. Let the blood go back to the brain, and that patient in five minutes will be as strong as ever, or as strong as before syncope took place. What happens when a patient has collapse? He goes into a weakened condition, from which he may be restored so that in a few minutes he may be as strong as he was before collapse occurred. That is not shock. What happens when you get a patient in shock? There is nothing on earth we can do to that patient that will restore his strength, except what? Give him time. Time is the essential factor in the recovery of shock. You can transfuse a patient, and he will make a brilliant recovery from his collapsed condition if it is due to hemorrhage; but, if he is in shock, nothing restores him but time.

Gentlemen, these conditions are not the same; they are most essentially different. Whatever the ultimate changes may be found to be, it is immaterial from a clinical standpoint—we have shock, and when we have true shock it takes time to recover from it. No drug will restore the patient. No stimulants will restore the patient. Time is essential to restore the nerve centers. It does not take much time to restore a patient in collapse or in syncope. He can be restored by stimulants,

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but he cannot be restored by stimulants if in a condition of shock.

DR. SEELIG (closing): Please do not misunderstand me. I regard it as my function not to furnish a theory as to the causation of shock, but, rather, to insist that the man who has attempted to furnish a theory, produces a correct one. I am as much in doubt about this problem as Dr. Harris is. I have not the first glimmer of a notion as to what the primary cause of shock is, and nothing, I think, shows better our confusion than the remarks of Dr. Andrews. I made no attack on Crile's treatment of shock. What I am objecting to is his insistence on the primary known cause of shock. When I spoke of Dr. Crile's stimuating influence, I referred to the stimulating influence that flows from the personal energy and enthusiasm which he puts into his work. With his fundamental hypothesis I do not agree.

Regarding the remarks made by Dr. Wetherill: It may warm the cockles of his heart to know that I have met several men, among them Dr. Corbett, who feel as he does, and I also share his opinion that, in all probability, shock is in a measure a conservative agency in much the same fashion that fever is a conservative agency. If fever gets too high, the patient dies. If shock is too profound, he dies; but we cannot reason that fever itself has necessarily only a noxious influence, for it may in a measure have a conservative influence.

As regards the question of Dr. Nifong, which relates to the existence of a symptom-complex, such as he described occurring several hours after operation: I should be disposed to speak of that type of shock in terms of hemorrhage. At all events, I should be inclined to think so unless autopsy proved otherwise. If an autopsy should prove otherwise, we would be forced to fall back on the delayed shock.

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REPAIR OF SMALL VESICOVAGINAL FISTULÆ

CHARLES H. MAYO, M. D., F. A. C. S.

ROCHESTER, MINNESOTA

The fact that vesicovaginal fistulæ are less common than formerly is undoubtedly due to better care given women during childbirth, though the condition is still occasionally seen as the result of prolonged pressure during labor. Another, or a newer, group of cases is the result of accidental or unavoidable injuries in performing hysterectomies.

Vesicovaginal fistula is a most disagreeable and troublesome infirmity because of the constant leakage, the attendant odor, and the local irritation. As seen by the surgeon these openings, large or small, appear to be mechanically easy of closure, yet the experienced operator knows that they are often very difficult to close and that they occasionally require repeated operations. Some of the openings about the ureter and the posterior wall of the bladder with fistulæ leading to the vagina are best approached by the abdominal route, extraperitoneally if possible, while the large vaginal fistulæ are best repaired by free incision and good exposure, which is often more difficult than it appears.

There are many cases in which the opening is so small as to be found with difficulty. For such and for those with a lumen not exceeding a quarter of an inch the following procedure, which I have successfully employed in a number of cases during the last twenty

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years, is recommended. The procedure is so simple and so generally successful that it almost partakes of a "trick" operation. I am personally indebted to the late Dr. Bernays for the principle of the operation, but it was devised by Dr. R. F. Amyx and first performed by him Dec. 17, 1902. It was demonstrated to Dr. Bernays the following year. (Fig. 1.)

Operation.—An incision is made through the vaginal mucosa extending completely around the fistulous opening about a quarter of an inch or less from its margins. The vaginal mucosa is dissected toward the opening, care being taken not to break through at the margin. This makes a little cup- or funnel-shaped opening projecting into the vagina. The circular dissection is carried deeper around the fistula, not approaching nearer than one-eighth of an inch to the margin, its depth penetrating to the mucosa of the bladder, but not through it. This leaves a little bell- or funnel-shaped opening lined with mucous membrane, which is connected with the mucosa of the bladder and projects into the vagina. (Fig. 2.) A ligature-carrier is passed through the urethra into the bladder and through the fistula into the vagina. A suture is passed through both walls of the funnelled mucosa on each side of the ligature-carrier. The two ends of the silk suture are now threaded into the ligature-carrier, which is withdrawn from the bladder and urethra. The ends of the suture projecting from the urethra are drawn upon, and with a little aid the fistulous tract starts inverting. (Fig. 3.) As soon as the mucosa disappears, a circular suture of fine chromic catgut is applied, a little more traction is used on the ends of the long suture, and a second purse-string suture of catgut is applied. The vaginal side is now closed either by a circular suture of the chromic catgut or by interrupted sutures, as seems best. This

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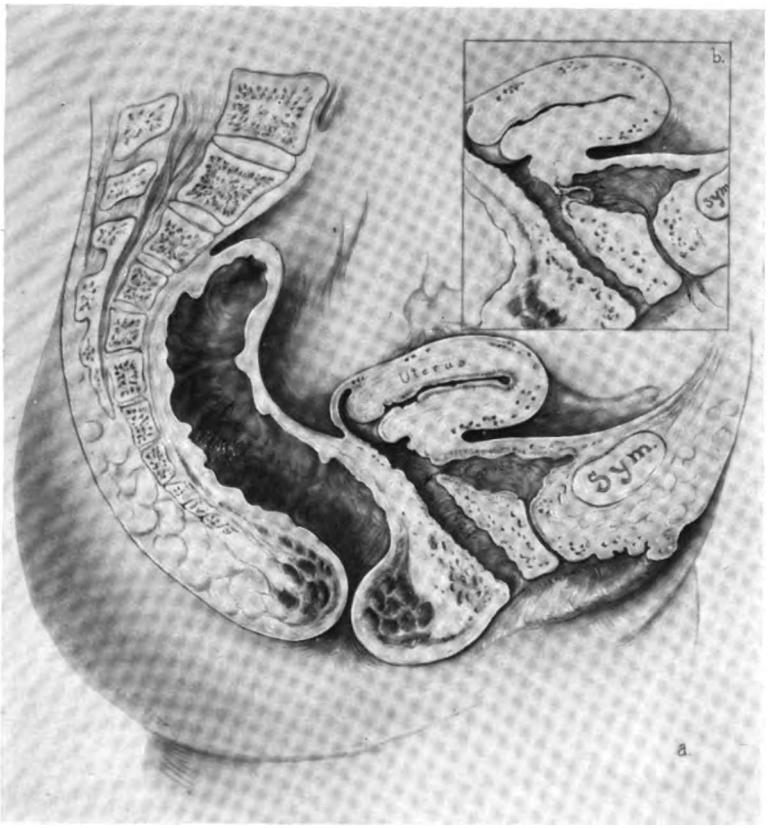

Fig. 1. *a.* **Vesicovaginal fistula.** *b.* **Inversion of vesicovaginal fistula.**

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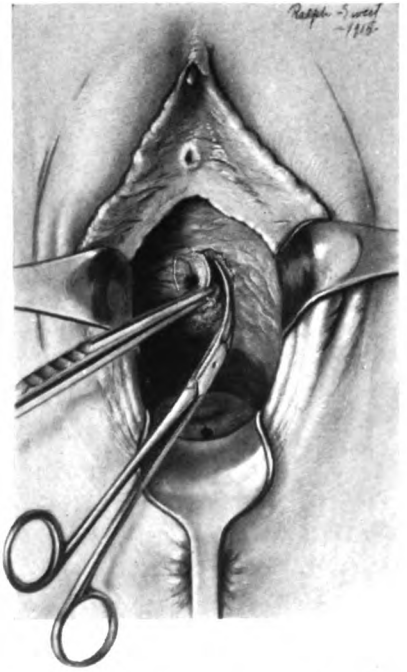

Fig. 2. Separation of vesicovaginal fistula.

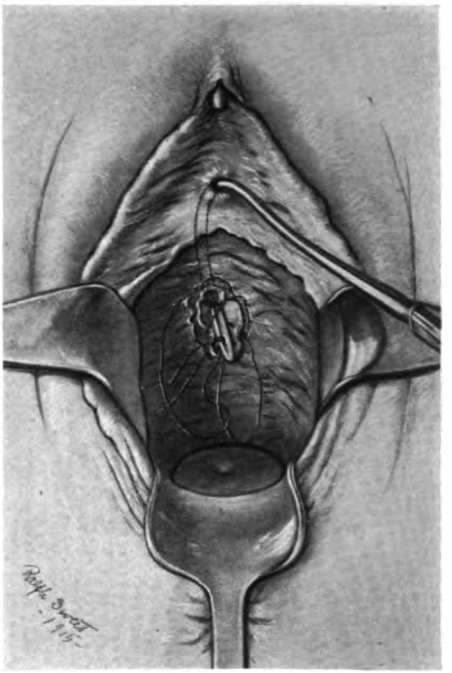

Fig. 3. Method of inverting fistulous tract by silk sutures.

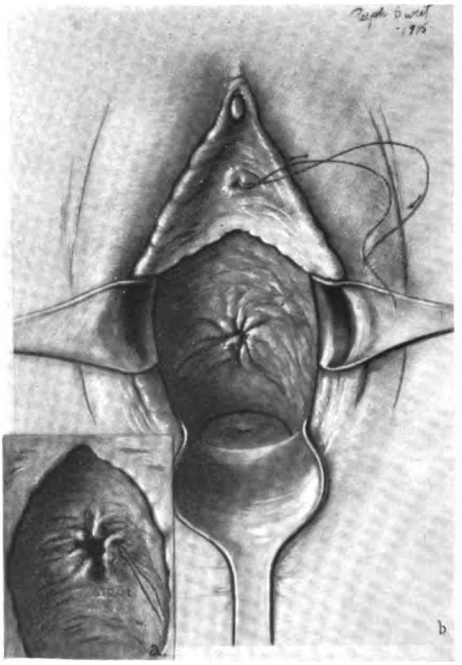

Fig. 4. *a.* Application of purse-string. *b.* Fistula closed.

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inversion turns the mucous surface into the bladder and leaves a healing surface within the tube. One of the long ends of the suture projecting from the urethra is re-threaded, and is sutured by a needle to the skin of the labia. The two ends are now tied at this point, making a slight traction. A self-retaining catheter (Pezzer type) is inserted into the bladder and the patient is instructed to rest on her side or even on her face. (Fig. 4.) This keeps the fistulous area free from urinary pressure. After four days it is necessary carefully to watch the catheter that sediment or phosphatic deposit does not obstruct its lumen. In some cases irrigation is necessary. However, the long suture attached to the inner side of the surface of the fistula and passing through the urethra acts as a safety-valve of leakage should the catheter become temporarily plugged. After a week the repair is usually solid, but it is better to keep the patient on her side or face for a few days longer that no undue strain may be placed on the fistulous area; and during this time it is best to keep a catheter in, or, if it is removed, to have regular periods for passing it. The suture from within the bladder either cuts itself out with the slight traction before it is time to remove the catheter or it may be drawn out without difficulty by cutting one side where it is attached to the skin.

DISCUSSION

DR. HENRY T. BYFORD (Chicago, Ill.): I would like to ask Dr. Mayo whether it is always necessary to keep in a catheter, and also to keep the patient part of the time on her face?

I have usually treated by Lawson Tait's method these small fistulæ. I split the septum near the mucous membrane of the bladder, turn the mucous membrane further into the bladder, and with a little scar-tissue on that side I apply sutures. Usually, instead of a circular suture, I should use a diagonal suture which will extend half the way across the fistula, half the way down, so to speak, over and back, so that two sutures

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cover the entire os, and a third one to close the ends, which would be beyond the bladder-opening. With this method I do not see why it is necessary to do any more, and I seldom leave a catheter in the bladder. If these patients do not urinate in three or four hours, then I introduce a catheter, and do not bother about position, except to keep the patient off the back; and the results are satisfactory. This method would be as safe, I suppose, as having these patients lie partly on their face merely as a matter of precaution, as recommended by Dr. Mayo. The point I want to make is, that the Tait operation, as described by him, is satisfactory, and I imagine a little easier to perform than putting this ligature through from the urethra.

Dr. James E. Moore (Minneapolis, Minn.): I had the pleasure of reading Dr. Mayo's paper, and it is so complete in its details and so satisfactory in the results related that it is impossible to offer any criticism. It reminds me very much of the days when this subject was a very important one. It recalls to my mind the days when Marion Sims made his reputation upon the closing of vesicovaginal fistulæ, which the men before him were unable to close successfully. About the same time Emmet introduced an operation for the repair of the perineum (perineorrhaphy), and a little later suggested an operation for lacerated cervix. I came closely in contact with all of his work.

There is one point I wish to call attention to in connection with the practical application in all of these operations now which they did not have then, and that is accessibility in doing the operation for vesicovaginal fistula. Dr. Sims, you may remember, invented a speculum which crowded the uterus up between the shoulders. Now we take hold, and pull it out of doors, so to speak. Nowadays we see a number of these cases, not from confinement, because we have trained obstetricians, but following vaginal hysterectomy. The secret of success is to make sufficient preparation between the bladder and the uterus, enabling us to draw the fistula down, and do the work comfortably and easily, putting in stitches accurately.

I have had very little trouble with the retained catheter. I have not much use for it in any of my cases. I prefer repeated catheterizations with aseptic instruments in the hands of a capable nurse. When a patient is on her back the danger is from intravesical pressure; and that pressure is alike in all directions, regardless of the position of the patient. When

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we operate upon a lacerated cervix nowadays, we do differently than Munde did when I assisted him. At that time, we reached away up with specula and long needle-holders, and had a hard time to succeed. Nowadays we pull it down, and that is the thing to do. We have gained as we have gone along.

Dr. James F. Percy (Galesburg, Ill.): I see a good many vesicovaginal fistulæ following my heat technic in inoperable cancer of the uterus. We shall probably see more of them as this technic becomes better known.

I have found that I can make the operation for their closure more easily and satisfactorily if I enlarge the field by the use of my vaginal dilator. It is much less difficult to map out and make the vaginal flaps when the vaginal walls are stretched taut by this instrument.

I make the flaps as suggested by Dr. Mayo, and dissect them down to, but not opening the edge of, the fistula. The flaps are then turned in and their raw edges fastened with chromic gut.

This is the only method that I know by which the mouth of the ureters, if exposed in the fistula, will not be deformed when the closure is made. If this deformity occurs, great damage may result to the kidney and ureter.

Dr. Mayo (closing): My paper dealt primarily with the small fistulæ. They are not larger than a match, and yet are very troublesome to deal with. I have never had a failure with this method. I got the idea from Dr. Bernays twenty-three years ago. Such a procedure is not in any way intended for those men who are successful in closing these fistulæ by some other method, because, if they are satisfied, I am; but this method is for those practitioners who have trouble.

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A PARADOX IN CANCER

CHAUNCEY E. TENNANT, M. D., F. A. C. S., AND
CASPER F. HEGNER, M. D., F. A. C. S.

DENVER, COLORADO

The case herewith reported presents an interesting sequence of etiological factors with associated carcinoma of the mammary gland. Paradoxically, it presents an equally important sequence of etiological factors with which carcinoma at the pylorus of the stomach is usually associated, but without the carcinoma being demonstrable. This paradox calls into question certain accepted factors predisposing to malignancy, and seems to refute the quite generally accepted factor of constitutional susceptibility.

In the case we report here nothwithstanding the long and continued presence of carcinoma, extending over a period of ten years, and two operations for recurrence elsewhere in the body; notwithstanding the possibility of metastasis and a constitutional predisposition to carcinoma; and notwithstanding the coexistence of a very chronic ulcer at a site favorable to the development of carcinoma, and that, too, during the cancer decades of life, carcinoma *did not develop*.

Is there a constitutional factor in malignancy? Are there bodies which prevent the development of carcinoma, or influence the degree of malignancy if carcinoma is present? Would these factors be sufficiently potent to overcome carcinoma in one locally predis-

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posed site, and not in another equally predisposed and co-existant site?

What factors favored the development of carcinoma in the one site, and what factor prevented its development in the other site? Is the duodenal site of an ulcer sufficient to explain the non-development of carcinoma? Can the anatomical and physiological peculiarities of the duodenum overcome all the other factors in this case?

Living pathology proves the frequency of duodenal ulcers as compared with similar gastric lesions. Post-mortem pathology does not confirm this observation. The apparent difference between these observations may be accounted for by the marvelous healing power of the duodenal mucous membrane, which conceals many of the ulcer scars, and by the migration of the unhealed and chronic duodenal ulcers toward the stomach. May these account for the rarity of duodenal carcinoma? What other influence is responsible?

CASE 1.—Mrs. A. L. McK., widow, aged 58. An elder sister died of cancer in 1913. She had the ordinary diseases of childhood. Since the age of 16 she has been having attacks of stomach trouble with nausea and vomiting. These have been severe and almost constant since 1905. She was married at twenty-two and had but one child, which nursed. During nursing the nipple of the left breast became sore, and abscess developed, and it was lanced through the nipple. In 1900, twenty-one years after the abscess, the same nipple became sore again. This soreness continued until in 1907. The nipple ulcer was diagnosed as one of Paget's disease, and was treated for a year and a quarter with the *x*-ray without improvement.

In March, 1910, which was about one year later, she was referred to me by Dr. E. B. Swerdfeger. There was at this time a superficial pigmented and fibrosed ulceration, one inch in diameter, about the nipple of the left breast. There was no evident glandular involvement. Dr. A. J. Markley, of Denver, was consulted. He confirmed the diagnosis of Paget's disease, and recommended excision of the nipple and

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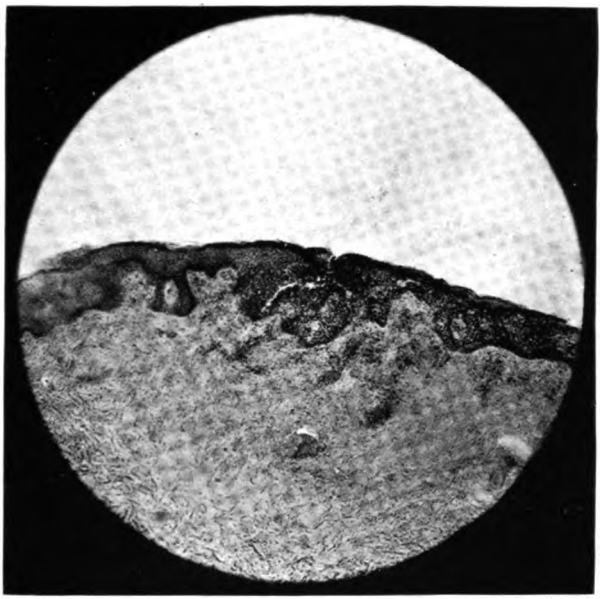

Fig. 1. Paget's disease (cancer) showing epidermal proliferation and overgrowth, broadening the interpapillary bodies and compression of the papillæ. In corium is seen reaction with infiltration of leucocytes, plasma cells, and new capillary formation. Keratinization is slight over altered area and pronounced in others.

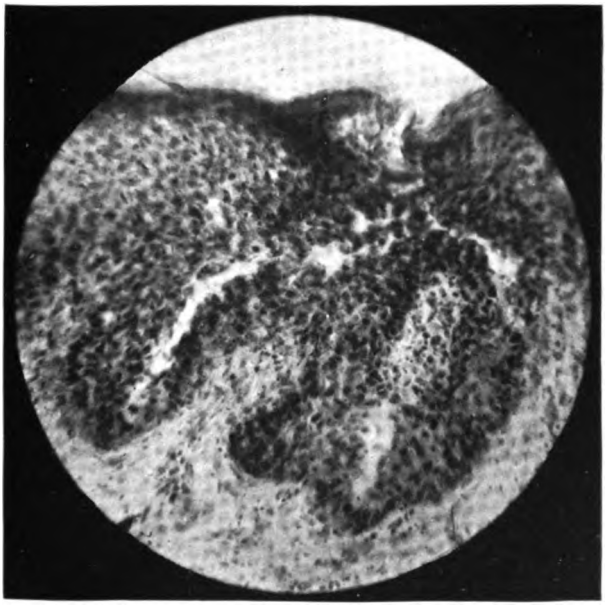

Fig. 2. Epidermal cells large, vacuolated and clear; nuclei swollen, irregular; intercellular spines indistinct. Keratinization is insignificant.

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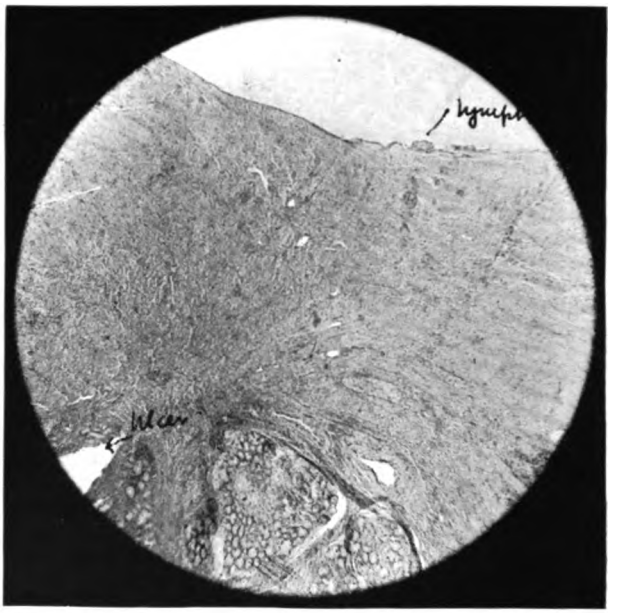

Fig. 7. Edema, new vessel-formation, and other inflammatory phenomena. Few tags of organized lymph on serous surface.

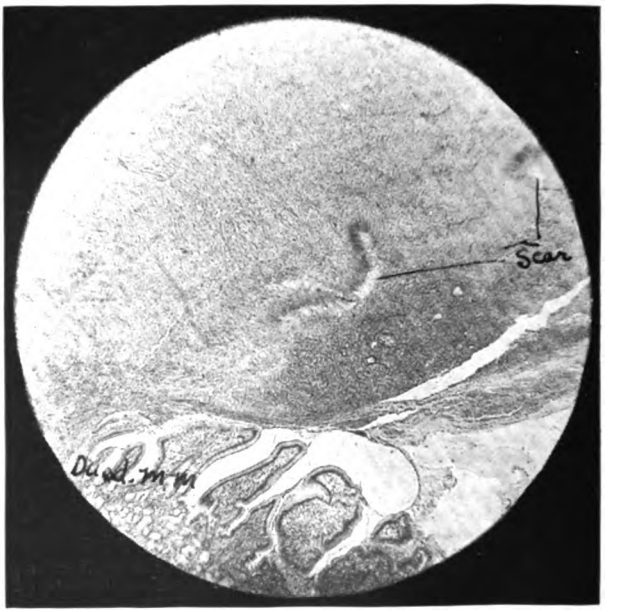

Fig. 8. Abrupt transition from necrotic area to epithelium of duodenal m.m.; absence of complication of the glandular tubules. Irregular, breastwork-like, richly cellular, scar-tissue formation, limiting the depth of necrosis.

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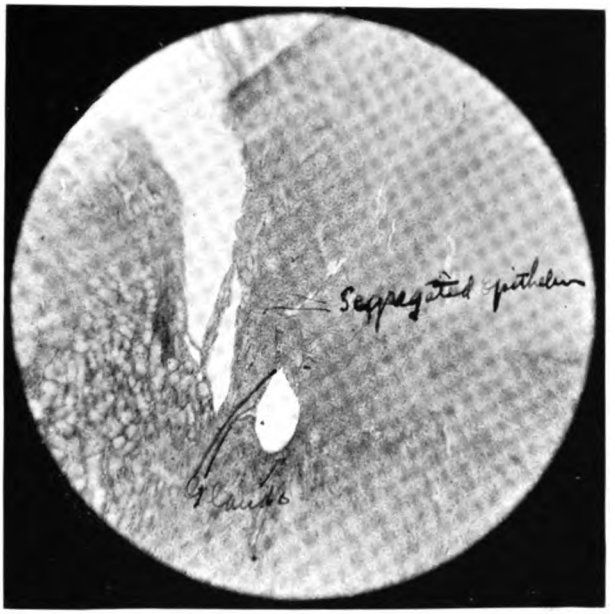

Fig. 9. Pyloric margin of ulcer, showing a more marked disarrangement of the glandular elements and some segregation of the epithelial elements, which do not show any suggestion of malignancy.

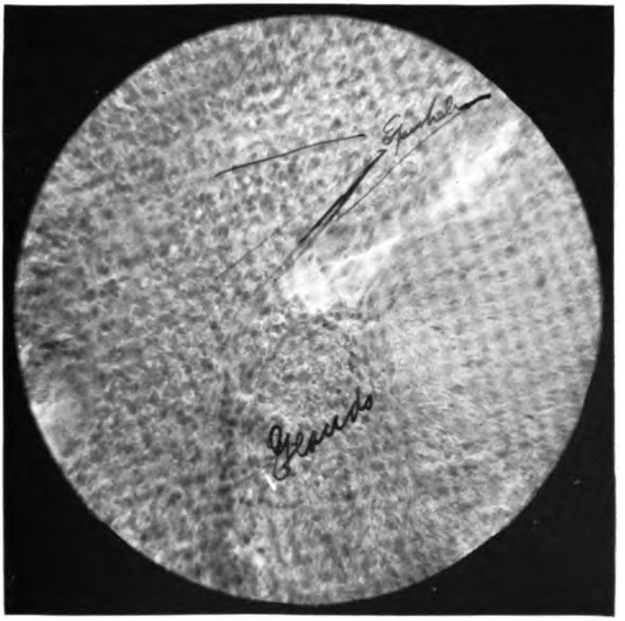

Fig. 10. Pyloric margin of ulcer, showing a more marked disarrangement of the glandular elements and some segregation of the epithelial elements, which do not show any suggestion of malignancy.

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ulcer area. This was done, and for the specimen shown here we are indebted to Dr. Markley.

In December, 1911, twenty-one months after the operation, the patient returned with a mass in the upper inner quadrant of the same (left) breast. It was about the size of a hen's egg, and adherent to the skin and deeper structures. Her attention was first attracted to this breast four months previously by soreness, pain, and a small lump. A complete operation was made, removing the breast, muscle, and supraclavicular lymph-glands. This material was submitted to Dr. R. C. Whitman, of Boulder, Colo., who reported scirrus carcinoma with glandular involvement. Up to the present time, or four years after this operation, the patient is free from recurrence.

In April, 1915, she consulted me for her stomach trouble, which had existed since she was sixteen years old, but which did not become marked until 1905, five years after the beginning of the Paget's disease. Within the last two years, and after the last breast-operation, it has become very severe. Her usual remedies (alkalies) failed to relieve her pain and distress, which were almost constant; acid vomiting was frequent, and she has lost ten pounds in weight.

Upon examination there was much rigidity and tenderness in the epigastric region with severe pain over the epigastrium.

On April 13, 1915, she was operated on. The pyloric end of the stomach, the pylorus, and the first part of the duodenum were fused into a mass, firmly adherent to the surrounding structures. The stomach (pyloric end) and the first portion of the duodenum were resected. The mass was excised in wide limits, and a posterior gastro-enterostomy was done.

The history and the appearance strongly indicated carcinoma, and at the time its presence was considered certain.

Convalescence was uneventful. The patient has gained in weight, and her stomach condition is better than for many years.

The case presented a number of interesting features in addition to the gastric condition. The more it was studied the more we found worthy of study.

The original lesion of Paget's disease existed, as the history shows, for a period of seven years, and

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persisted until the nipple and areola were practically destroyed. This in spite of treatment with *x*-ray under very competent supervision.

The specimen No. 1 (Paget's disease) shows the epidermal proliferation beginning in the prickle or spinous cell-layer and overgrowth, with broadening and lengthening of the interpapillary bodies, and compression and obliteration of the papillæ. In the corium is seen an inflammatory reaction with infiltration of leucocytes, and plasma cells with new capillary formation. Keratinization is slight over the area of epidermal activity, and pronounced over the unaltered areas.

No. 2 is a higher magnification of the area of epidermal activity. The cells are large, vacuolated, and clear. The nuclei are swollen, vesicular, or irregular. The intercellular spinous processes are absent or indistinct. Keratinization is indistinct.

No. 3 shows frank adenocarcinoma of the tumor removed at the second operation. Lymph-glands in axilla and above the clavicle showed metastases of similar character.

No. 4 is a view of the mucous surface of the pyloroduodenal transition. Normal appearance is obscured by the contraction of the scar of the ulcer and inflammatory infiltration.

No. 5 is the ulcer bisected, the pyloric portion being much firmer and more infiltrated than the duodenal.

No. 6 shows how the ulcer is concealed by the approximation of the edges of the mucous membrane, the pyloric margin being much more rigid than the duodenal.

No. 7 shows the edema, new vessel-formation, and other inflammatory phenomena, and a few tags of organized lymph on the serous surface.

No. 8 shows the abrupt transition from necrotic

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area to epithelium of duodenal mucous membrane, absence of complications of the glandular tubules, and irregular, breastwork-like, richly cellular scar-tissue formation limiting the depth of necrosis.

No. 9 is of the pyloric margin of ulcer, showing a more marked disarrangement of the glandular elements and some segregations of the epithelial elements, which do not show any suggestion of malignancy.

FOR DISCUSSION SEE PAGE 201

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CANCER OF THE RECTUM

ALBERT E. HALSTEAD, M. D., F. A. C. S.

CHICAGO, ILLINOIS

Anatomy.—The rectum proper, pars pelvius recti of Waldeyer, has for its upper limit the lower border of the third sacral vertebra. Its lower limit is the attachment of the levator ani muscle. In the male this corresponds to the lower pole of the prostate gland; in the female, to the middle of the posterior wall of the vagina.

This portion of the rectum, called by English anatomists the *second* portion, is derived from the highly differentiated portion of the hind gut, the cloaca. It differs from the third portion, the pars perinealis recti of Waldeyer, in its origin, the latter being derived from the ectoderm, and is formed by infolding of the epiblastic structures. This portion is subdivided by Waldeyer into three zones: (1) zona anocutanea; (2) zona intermedia; (3) zona columnaris.

The blood supply of the true rectum is derived from five sources, all of which are accompanied by lymphatics. The principal vessels are the superior rectal and the middle hemorrhoidal. The lymphatics accompanying these pass to the anorectal, sacral, and mesosigmoid glands, communicating above with the iliac and aortic groups. These lymphatics are rather inconstant in their number and distribution. The lymphatics of the anal canal, particularly of the anocutaneous portion, pass along the inner surface of the thigh, and

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reach the inguinal glands, mostly the internal and inferior groups. Those form the intermediate zone, communicate with those of the anus, and also pass directly to the glands lying beneath the fascia propria of the true rectum, the anorectal group. A few pass along with those of the median hemorrhoidal artery, and a few terminate in the glands at the ischiatic foramen.

The anorectal glands are of the greatest importance because they are the ones first affected in carcinoma of the true rectum and of the upper parts (the third portion) of the anal canal. They number five or six, and extend from the attachment of the levator ani to the reflection of the peritoneum from the anterior surface of the rectum. One, usually of large size, may lie laterally to the rectum, underneath the reflected portion of the peritoneum. This anal portion furnishes about 6 per cent of the carcinomas. They are frequently squamous-celled epitheliomata.

The first portion of the rectum, as described by American authors, is in reality the pelvic colon (colon pelvium of Jonnesco). It is derived from the hind gut, and has its lymphatics pass by way of the mesosigmoid to the aortic and epigastric glands.

FREQUENCY OF RECTAL CANCER

In order to arrive at some idea as to the frequency of rectal cancer, it is necessary to investigate the statistics of large hospitals.

In a series of 4,567 cases of cancer in the Vienna General Hospital, 143 were cancers of the rectum, and 35 of other parts of the intestine. This would give about 3 per cent for cancers of the rectum, and .76 per cent for those of other portions of the intestinal tract.

In the London General Hospital, out of 1,908 cases of cancer, 58 were of the rectum, giving about the

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same percentage as the Vienna statistics. The same relative number were found in 4,000 cases reviewed by Ball from the records of St. Mark's Hospital of London.

We may assume that cancer of the rectum represents about 3 to 3.5 per cent of all cancers, and about 80 per cent of the cancers of the intestinal tract.

As regards sex: It is frequently stated that men are affected about twice as frequently as women. This is not borne out by the statistics above quoted. In these the disease affected both sexes with about equal frequency.

Age.—In general, cancer appears most often between the ages of 40 and 50. The youngest that I have found in the literature was 13, reported by Gowlland, in the Reports of the St. Mark's Hospital. In my own cases there was one, a male, aged 18, and a female, aged 19. The others were all above 30.

Symptoms.—The symptoms depend somewhat upon the location and character of the growth. In *low* cancers, particularly when the anal canal is the seat, *pain* may be an early and prominent symptom. In *high* cancers, involving the rectum proper, situated in the ampulla, where at least 75 per cent originate, pain may be insignicant for a considerable time.

Hemorrhage is often the *first* and may for a long period be the *only* symptom.

In this connection I will warn you that where a patient complains of hemorrhage from the rectum, no matter how insignificant, it is worthy of the most careful investigation. In the last year two patients have consulted me who were found to have well-advanced carcinoma of the rectum, who had been treated by reputable medical practitioners for months for intestinal hemorrhage without ever having had a rectal examination. One had progressed so far that only a

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colostomy could be considered. Both had large carcinoma within easy reach of the examining finger.

In the large proportion of the cases that I have seen the patients have first sought medical counsel because of a discharge of blood from the rectum.

Constipation lasting for a long time, alternating with diarrhea, is a common symptom in the disease. As the growth progresses, frequent evacuations, followed by a discharge of blood and mucus, is the rule. As obstruction symptoms develop, abdominal pain, vomiting, tympany, and loss of flesh ensue.

Enlargement of the inguinal glands should, in every case, direct attention to the anal canal as a possible location of the primary growth. In two cases seen by me, the glandular enlargement was noticed long before the malignant growth in the anus was recognized. In one the diagnosis of cancer was made, and the primary growth located in the anus after microscopic examination of exised glands revealed their cancerous nature.

The diagnosis is made on the above-mentioned symptoms with finding, on rectal examination, of a hard nodular growth by means of the examining finger. Any growth within six inches of the anus may be felt on bimanual examination. A small epithelioma hidden in a mucocutaneous fold in the anal canal may not be recognized by touch. A careful inspection under good light is essential in the recognition of this variety of growths.

The use of the proctoscope is essential in the location of high carcinomata, those at the upper limits of the true rectum or in the pelvic colon. However, the chief reliance must be placed on the ability to recognize by *touch* any abnormality in the rectal mucosa. Inasmuch as 70 per cent of all carcinomas of the rectum occupy the ampulla (the lower part of the true

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rectum), and 10 per cent are below this point, it is apparent that in 80 per cent of the cases the diseased portion of the bowel may be readily palpated. In the 20 per cent lying above the ampulla, probably three-fourths may be reached by the examining finger, if the patient be anesthetized. The recognition of the few remaining cases may require the use of the proctoscope, with Röntgen ray examination.

The type of cancer found in the rectum proper is adenocarcinoma, most cases being of the ordinary, simple forms. In a few cases we have found papilliform or villous adenocarcinoma. The latter may puzzle the inexperienced when an examination is made. In place of a hard nodular growth or an ulcer with an indurated border, we find a soft pulpy mass that bleeds easily and is nowhere indurated. Visual examination through the proctoscope will permit of easy recognition of the character of the growth.

Cancer of the rectum is of rather slow dissemination, and remains for a considerable time a local disease. Metastasis through the lymphatics is, as a rule, long delayed.

Oehler (Bruns' Beiträge, Bd. 87, 1913) has recently reviewed this subject, and has examined post-mortem 58 cases from Kraske's clinic. In 34 no internal metastases were found.

Zinner also found that in 141 deaths from cancer of the rectum, the retroperitoneal lymph-nodes were the seat of secondary growth in only 5 cases (3.5 per cent).

These findings are of interest from an operative point of view as they present a strong argument against the abdominal or combined abdominal and low operation in the ordinary types of cancer located in the true rectum. The chief argument used by those who advocate the more dangerous abdominal pro-

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cedure is, that it gives the operator a chance to remove the entire field of glands that may be affected. It goes without saying that even in the most favorable cases this could not be accomplished. The mortality in the abdominal or combined operations is about 66 per cent (Heiler: Ergebnisse d. Chir. u. Orthöp., Bd. 5, 1913), while in the sacral it is about 15 per cent (Lusk and Hochenegg's review of 1,244 cases). The final results are also better in the Kraske type of more favorable cases for the low operation.

The abdominal or combined operation should be reserved for those situated more than eight inches above the anus. These are in reality cases of cancer of the colon.

PERSONAL EXPERIENCE

Since 1895, I have treated 63 cases. Of these, 7 died without any operative treatment, 3 from general peritonitis due to perforation of the bowel, and 4 from cachexia and exhaustion incident to advanced carcinoma.

In 20, colostomy with establishment of permanent artificial anus was the only operative procedure considered indicated.

In 9, perineal resection was done after the methods of Lisfranc, Dieffenbach, and Tuttle.

In 18, posterior resection after Kraske or some modification.

In 4, combined abdominal and perineal resection.

In 2, combined abdominal and posterior resection.

In 1, abdominal resection.

In 2, resection of a prolapsed rectum, the seat of adenocarcinoma.

Colostomies:

Ten died within one year; 4 lived less than two months; 6, more than two months, but died within a

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year. Three lived between one and two years, and 4 lived more than two years (one, two years and one month; one, two years and five months; one, two years and eight months; and one seven years after colostomy).

Three were not traced. Duration of life not known. These lived to leave the hospital in from three to six weeks after the artificial anus was established.

Perineal Resection: Lisfranc or Dieffenbach type of operation, 9 cases.

Immediate mortality, none.

Deaths, 3 cases within one year.

Two cases lived two years or more. Not traced after two years.

One lived from date of operation, in 1899, to 1912, or thirteen years; when last seen, 1912, he was in good health.

Three cases could not be traced after leaving hospital.

Eighteen posterior operations, following general plan of Kraske (resecting coccyx and more or less of sacrum, up to the third sacral vertebra).

Of these 18 posterior operations, 5 were done without preliminary colostomy. In 4, an effort was made to retain control of the sphincters by following the method of Dieffenbach. The general plan of the operation was as follows:

The rectum was exposed by an incision passing from the anus to the second sacral vertebra. The coccyx and two sacral vertebræ were excised. The rectum was dissected from its surrounding structures, double clamped below and above, and divided between clamps by means of the cautery-knife. The mucosa was dissected from the anal stump, and the proximal cut-end of the gut brought down and stitched to the anal verge, after the gut had been rotated 180 de-

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grees, so as to bring the anterior peritoneal coat posterior to facilitate rapid healing. Fair sphincteric control was secured in two cases. In two, incontinence without any control resulted. If the levator ani is so conserved in the operation that it is possible to insert it by suture into the rectum, and its nerve supply remains intact, good control may be expected. In one case of perineal resection this feature of the operation gave very fair control, even after resection of both sphincters.

In one of the posterior resections a sacral anus was established, using the procedure of Gersuny, to establish a sphincter-like control by twisting the gut on its transverse axis. This was a failure, the sacral anus being in a position where it could not be cared for properly. The functional result was very poor.

Here I would warn against the *sacral* anus. It is inferior to a well-planned inguinal anus. In the latter, if properly established, we may expect almost complete control of the bowel movements. Even gas may be controlled to a considerable extent. The most important point in its favor is, that it is so placed that the patient can care for it with the least inconvenience.

In 13 cases *preliminary colostomy* was performed, the posterior resection being carried out, as a rule, from two to three weeks after the artificial anus was established. In these cases we had two deaths following the operation. One resulted from a general peritonitis due to a partial separation of the colon from the abdominal wall at the site of the colostomy opening. This resulted from traction on the colon in resecting a high carcinoma through the posterior incision. The damage was not recognized at the time. Leakage consequent to the separation was followed by peritonitis, which proved fatal on the third day.

In the second case, death resulted from hemorrhage

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following resection of the rectum and a portion of the bladder-wall that was found at the time of the operation to be involved by extension of the carcinoma. At the time of the resection this patient was greatly reduced by hemorrhages, which continued after the colostomy was performed. I now consider that this case should never have been subjected to a radical operation.

I am firmly convinced that all posterior operations for cancer should be preceded by a colostomy. This gives the patient an opportunity to gain strength to withstand the extremely serious operation that is to follow. It also enables the surgeon to explore the abdomen and to determine thereby the existence or absence of liver and glandular metastases, thus preventing his performing a useless radical operation on cases where metastases exist. I hold that *any* invasion of the peritoneum, retroperitoneal glands, or the liver, absolutely contra-indicates any radical procedure. This is also true of extension to the bladder, seminal vesicles, and prostate gland, and to the uterus in the female when such extension can be demonstrated before operation.

In 4 of the 13 cases in which a preliminary colostomy was done, the proximal end of the resected rectum was loosened and drawn down to the anus after the colostomy opening was closed. In one of these this was done in six months, and in another eleven months, after resection of the rectum. In neither was any appreciable control regained. In two in which the resected end of the rectum was attached within the anal canal at the time of the resection, fair control was obtained.

In 9, no attempt was made to re-establish a perineal anus. The colostomy was permanent.

Of the 16 posterior operations that survived the

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immediate effects of the operation, only 14 could be traced after leaving the hospital. Seven of these have been operated on during the last three years; 3 died within one year; 1 in one year and three months; three are still living, without recurrence. One has a recurrence, recently developed. This patient was operated on fifteen months ago, six inches of the rectum being excised. There was no recurrence for one year. Recently, within one month, examination reveals a local recurrence in the neighborhood of the prostate gland.

Of the 7 remaining, operated on more than three years ago, 4 died, 1 in nine months, 1 in fifteen months, 1 in sixteen months, and 1 in twenty-one months after radical operation was performed.

Three are alive: 1, seven years; 1, four years and two months, and 1, three years and three months after operation.

Of the 4 operated on by combined abdominal and perineal, 1 died as result of the operation. This death was due to an accidental injury to the bladder, which was not recognized. Leakage with peritonitis followed.

Two died: one six months after operation from recurrence at site of the growth; and one sixteen months after operation from metastasis in the liver. No evidence of metastasis was discovered at the time of operation. One case operated on in 1900 is still alive, sixteen years after the operation.

In this group all the operations were performed in one stage. The abdomen was opened, and inspected carefully for evidences of metastasis. The colon was divided through the lower part of the sigmoid loop. After doubly clamping it, it is divided by means of the cautery-knife. With the patient in the Trendelenburg position, the abdominal cavity was carefully packed off. The cut-end of the lower fragment is

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turned in with a purse-string suture. The clamp on the proximal end is allowed to remain after it is carefully wrapped in gauze. The inferior mesenteric in high carcinoma and the superior hemorrhoidal in lower ones, is ligated. After freeing the gut from the mesocolon and the tissues in the hollow of the sacrum, the middle and lateral sacral vessels are secured.

The middle hemorrhoidals are clamped, and then ligated.

When the attachment of the levatores ani is reached, the distal fragment of the colon and rectum are invaginated into the rectum, and the pelvis wiped clean, a drain is inserted down to the bottom of the wound, and the position of the patient is changed to that of the exaggerated lithotomy position.

Resection by the Lisfranc, or some modification of it, completes the removal of the entire rectum and pelvic colon.

The proximal end of the colon is fastened into a lateral opening. The median incision is closed, except when drainage is inserted.

In one case a high carcinoma was removed through an abdominal incision. The pelvic colon was dissected free, with the upper portion of the rectum, to within an inch of the prostate gland. The lumen of the lower fragment was closed by means of a suture. This patient made an uneventful recovery from the operation, lived two years, and died from local recurrence.

In two cases of prolapsus recti an adenocarcinoma was found at the apex of the prolapsed invaginated rectum. Resection after the method of Thiersch was followed by recovery. Both patients are alive at the present time, one seven years and one four years after the operation.

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MALIGNANT DISEASE OF THE OVARIES

FURTHER STUDIES

MILES F. PORTER, M. D., F. A. C. S.

FT. WAYNE, INDIANA

Two years ago I read a paper before this Association on the subject of sarcoma of the ovary. Since that time there have occurred in my practice three cases of malignant disease of the ovary. The occurrence of these cases prompted the further inquiry into the subject of malignant diseases of the ovary, and prompted me to write this paper.

CASE 1.—Mrs. G., aged 33, came to me complaining of enlargement of the abdomen. She had been married twice, and had one child, aged 16. Her general health had been good. I had operated on her seven years ago for a large papillary cyst of the left ovary complicated with pus tubes. She made an uneventful recovery, and enjoyed good health. She has not menstruated for two years. Three years ago she first noticed the enlargement of the abdomen. Two months prior to her last visit to me she had had some irritability of the bladder. One maternal uncle and one maternal aunt died of cancer.

Examination revealed a large pelvic tumor about the size of a seven month's pregnancy. The scar of the previous operation was quite thin. There was a large lymphatic gland in the right inguinal region the size of a hazelnut. This was said to have appeared three weeks ago as the result of a fall. She was operated on, September 8, and a large multilocular cystic tumor of the right ovary removed. The adhesions were very extensive and close, necessitating the baring of the iliac vessels and the ureter during the operation, together with a removal of part of the uterine wall to which the tumor

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was attached. The recovery was uninterrupted and the patient remains entirely well up to date. It should be added that there was no ascites.

Unfortunately this tumor was mislaid and destroyed, so it was not subjected to microscopic examination. A particularly interesting feature in this case was that seven years prior to this operation the patient had the opposite ovary removed for a large cystic tumor which was pronounced by the pathologist, Dr. Rhamy, to be a benign papillary cyst. That the pathologic diagnosis of this first specimen was correct I think the history of the case proves. I myself do not think there was any connection between the first tumor and the last. The last tumor removed was, as nearly as I can judge from the gross specimen, a malignant tumor of the ovary. Unfortunately this question cannot be settled unless there should be a recurrence, which would result in a microscopic examination of the growth. The bilateral tendency of ovarian growths is well recognized, and, indeed, has led some men to advise removal of both ovaries whenever the removal of one becomes necessary because of tumor formation.

Silhol[1] asserts that this bilateral tendency is quite as well marked in innocent as it is in malignant tumors. Clark, in commenting on Silhol's paper, thinks that Silhol's statement is unnecessarily radical; but further on in the course of his remarks, he says, "of course, if the original unilateral lesion shows the least suspicion of malignancy, the question assumes a different aspect at once, this only emphasizing, however, the great importance, especially in ovarian surgery, of a thorough knowledge of living pathology on the part of the operator." My experience is quite large, covering hundreds of cases, and I cannot recall ever having been called on to remove a cyst of the remaining ovary after an ovariotomy had been done for like trouble

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save in the case reported above. I am, therefore, inclined to take an even more conservative stand on this subject than does Clark. I should not advise the removal of what appeared to be a healthy ovary, even if its fellow was the subject of malignancy, provided the malignant disease seemed to be confined to the ovary removed. Certainly it seems to me that this should be the rule which should guide us in operating on young women. In women near the menopause there could perhaps be little objection to removing the remaining seemingly healthy ovary.

G. W. Perkins,[2] in a paper in which he analyzes the reports of the Massachusetts General Hospital between 1870 and 1910, reaches the conclusion that "fluid in the abdomen of a child without general anasarca, provided pericarditis and cirrhosis could be excluded, should always be investigated by exploratory laparotomy."

As a result of personal experience, illustrated by the following case, I should be inclined to amend his conclusion by substituting the word "woman" for "child."

CASE 2.—Mrs. J. G. B. J. consulted me, Sept. 15, 1915, complaining that her abdomen had been filling up and that there was epigastric distress after meals which was somewhat relieved by food. She had had malaria frequently when a child. She had been married for seventeen years. One child was prematurely born at seven months. During this pregnancy she suffered severely from albuminuria, and was quite dropsical. Menses were regular and normal at three to three and one-half weeks. She was subject to sick headaches. The family history was negative. The present trouble commenced with abdominal distress about two and one-half months ago; bloating was severe, and the bowels were kept open with medicine. There was some swelling of hips, thighs, and ankles, but this had gone down. The patient gained in weight, but the extremities were thin. Examination revealed a very much enlarged abdomen with fluctuation, and dulness

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on percussion below and at the sides with the patient on her back, the line of dulness changing with a change in position of the patient. The examination of the urine, blood, heart, and liver revealed no cause for the ascites. The Wassermann examination was negative.

An exploratory operation revealed the abdomen well filled with an amber colored fluid and containing a right ovarian cyst about the size of a cocoanut. The omentum was somewhat shortened and thickened. The opposite ovary was healthy. The cyst was torn during delivery, and a very small part of the contents escaped. Prior to the operation a bimanual examination failed to reveal any sign of this tumor, which proved on examination to be a carcinomatous cyst. It may be of interest to note that, with a view to preventing the return of the ascites, I stitched the omentum into the abdominal incision throughout practically its whole length.

Up to the present writing the woman remains perfectly well. It is, of course, probable that she will have a return of her trouble. However, there was no evidence of involvement of any of the other abdominal organs. The only excuse I had for operating on this woman was that I found an ascites for which I could find no cause. The operative findings proved the wisdom of the course pursued, and strengthened me in the position that I have long held, namely, that fluid in the abdomen should lead to an exploratory operation when the presence of the fluid cannot otherwise be accounted for, and that in women this dictum should be well-nigh imperative.

Case 3.—Mrs. C. G., housewife, aged 48, came to me, July 29, 1915, complaining of a lump in the lower right abdomen. She had malarial fever three years ago. The tonsils had been removed and the nose operated on twenty-two years ago. The menses came at thirteen, and were regular until last August, at which time, following the removal of a small fatty tumor from the breast, the menses became irregular in that there was a decided tendency to flow too frequently. Since Easter she had been flowing every day, but never very profusely. She had been married eighteen years,

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and had had three children; the youngest was 15; she had had no miscarriages. The family history was negative. The patient said she did not feel sick, but had lost some flesh. The bowels were constipated. The appetite was fair. Examination revealed an even, smooth, firm, movable growth apparently attached to the anterior right aspect of the uterus. I considered the case to be one of fibromyoma and advised its removal. The patient consented to have it done, but wanted to go home and await a more convenient time for having the work done.

About three months later she was taken suddenly ill with pain in the lower right abdomen which required morphine to allay. There were some nausea, chilly sensations, and hot flashes. I found on inquiry that she had two or three similar attacks, but none of them very severe. On examination the uterine tumor seemed to be much larger than when I last saw her. The whole abdomen was tender, especially the lower two thirds. The pulse was somewhat quickened, the temperature 102.°

The patient was put to bed with an ice-bag on the abdomen, and allowed no food for three days, when the more acute symptoms subsided, and the abdomen was opened, revealing a right ovarian tumor twisted clockwise three-fourths of a turn on its pedicle. There was little fluid in the abdomen, slightly tinged with blood. The tumor itself was solid, seemed to be enclosed in quite a perfect capsule, and was dark colored in places owing to extravasated blood consequent on the twist. The tumor on section proved to be solid and mottled, the prevailing color being yellow with some dark-red spots as a result of hemorrhage into the substance of the growth. The upper third of the tumor was darker, owing to the fact that in this area there was more extravasation of blood, because of the twist in the pedicle than in any other parts of the tumor.

The pathologist's report was adenocarcinoma with hemorrhagic infiltration. (Fig. 1.)

The gross appearance of this tumor on section is so significant of malignancy that a laboratory report to the contrary would not be accepted unless it was based on a careful examination of the entire tumor; and then I should want to wait until time for further

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clinical manifestations had gone by before concluding that it was benign.

Speaking of tumors generally, I should say that negative reports from the laboratory when based on a few sections should never be allowed to outweigh the evidence gained by the examination of the gross specimen plus that obtained from the clinical history. On the other hand, one frequently meets tumors which present little or no macroscopic evidence of malignancy, but which on microscopic examination show unmistakable evidence of malignancy. A case occurring in my own practice recently will serve to illustrate this point:

CASE 4.—An elderly woman presented herself with a tumor of the breast, the character of which I regarded as doubtful. The breast was removed, and on section the mass was found to be made up chiefly of two cysts the size of a chestnut, distinctly separate, movable, and containing each a little slate-colored grumous fluid. My doubt as to the malignancy of the tumor was so great that I felt warranted in closing the wound while waiting for the pathologist's report. About the time the wound was closed the pathologist reported that the tissue did not look good to him, whereupon the usual radical operation was done. Later on I received a further report from the pathologist in which he says, "A very interesting specimen, as the microscopic picture of malignancy is out of all proportion to the macroscopic."

It will be remembered that in Case 3 and in Case 1 there was no fluid in the abdomen except what might be accounted for by the twist in the pedicle. These are not the only malignant tumors of the ovary that I have found without ascites. It should be remembered, therefore, that while ascites in connection with ovarian tumor is an indication of malignancy, the absence of ascites in ovarian tumors does not necessarily mean benignancy. It is well here to emphasize the point that ascites developing in connection with

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malignant tumor of the ovary does not mean necessarily an involvement of the peritoneum in the malignant process. Just why some of these tumors are accompanied by ascites while others of them are not remains unexplained. Not infrequently, indeed, as in Case 2, reported above, the ascites may be the only or the principal sign of trouble. My own belief, which is borne out by my experience, is that the rapidly growing tumors are the ones most likely to cause ascites.

G. Kline[8] found on microscopic examination of two malignant papillomas of the ovary immediately under the surface epithelium of the villi, and only there, that the connective tissue was saturated with serous fluid. "This was due to absorption on the part of the tumor epithelium which had taken up fluid from the lymph spaces of the abdominal cavity. This fluid may possibly have a toxic effect on tumors. If this is true, the appearance of ascites in malignant tumors is to be regarded as a protective procedure on the part of the body designed to destroy the tumor. Therefore, the subcutaneous injection of ascitic fluid may be regarded as a rational therapeutic procedure." Concerning the influence of malignant ovarian tumors on the menstrual function, cases are encountered wherein they seem to cause amenorrhea, and others wherein they seem to produce menorrhagia or metrorrhagia. These opposite effects on the menstrual function may be explained very logically, it seems to me, by the overgrowth of ovarian structure, on the one hand, or its destruction, on the other. However, it is found that some patients suffering from malignant ovarian tumors present a history of amenorrhea followed by menorrhagia or metrorrhagia or vice versa. The explanation for this condition is to me not apparent. In the case-histories which I went over in my former paper,

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I found two patients who had a bloody vaginal discharge which came on years after the menopause. One of my own cases reported in that paper was that of a girl of fifteen, who had never menstruated though she apparently was quite mature for her age. In this patient both ovaries and uterus were involved in a large sarcomatous growth. (It is interesting to note in this connection that this patient, now more than two years and six months after the operation, is still in perfect health.)

In Case 1, reported above, the patient menstruated regularly for five years after the removal of the first tumor, but had no menstrual flow for two years prior to her last visit to me. In Case 2 the menses were normal. In Case 3 there was some menorrhagia. I am inclined to attribute this symptom in this case, however, to the twist in the pedicle. I think we are warranted in concluding that either amenorrhea or menorrhagia is much more common in malignant tumors of the ovary than in benign tumors of the ovary, and that both forms of irregularity are equally indicative of malignancy. In my former paper I quote Wermuth as saying that sarcoma in the young may cause premature signs of puberty, and added that I had not been able to find any other author who confirmed this statement, and that it seemed to me problematic. Since that time I have run across a report of the following case by Verebely[4]:

> A girl, six years old, up to five years of age had developed normally. Since a year hemorrhages had recurred monthly, and other signs of puberty had appeared. The child was about four inches taller than one of corresponding age, and the breasts, which were the size of lemons, contained glandular structure. In the axillæ, as well as in the pubic regions, there was an abundant growth of hair. The labia were thick and pigmented, and the vagina of abnormal size. Examination of the abdomen revealed a nodular tumor of the size of

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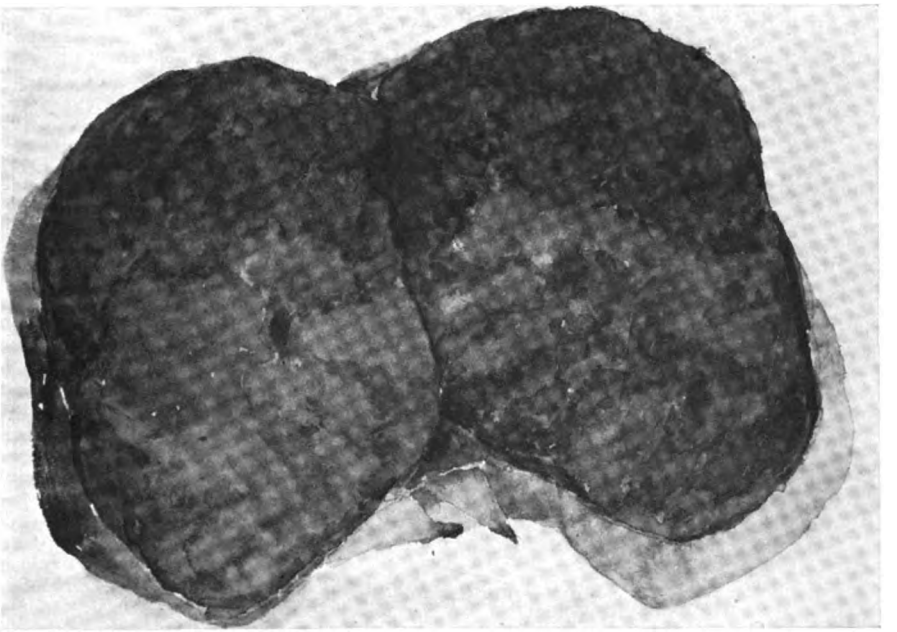

Fig. 1. (Case 3, Mrs. G. C.) Adenocarcinoma of the right ovary. Actual size. The tumor was split in the middle, and the picture was made from the fresh specimen. Note the deeper color of the upper third due to extravasated blood consequent on a three-fourth clockwise twist of the pedicle. Note also the distinct capsule.

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a child's head connected with the left side of the uterus. At the operation a very vascular ovarian sarcoma was found. The uterus was of the size of that of an eighteen or nineteen-year-old girl, the right ovary being normal.

REFERENCES

1. Silhol: Progr. Med., 1914, ii, 247.
2. Perkins, G. W.: Internat. Abstr. Surg., July, 1914, p. 54.
3. Kline, G.: Internat. Abstr. Surg., July, 1914, p. 54.
4. Verebely: Sajous's Analytic Cyclopedia of Practical Medicine, 1915, vii, 256.

DISCUSSION ON THE THREE PRECEDING PAPERS

Dr. B. Merrill Ricketts (Cincinnati, Ohio): I rise to speak principally upon Dr. Halstead's paper, although I have enjoyed all three of them. The data presented in his paper are a great addition to what we already have upon cancer of the rectum. I feel that there are one or two points that have been sadly neglected with reference to the influence of the coccyx upon the rectum and what it has to do with the excision of carcinoma or epithelioma of the rectum. The study of the coccyx has been uppermost in my mind for a number of years, also its comparative study.

If you will notice, the coccyx makes a curve forward normally, and it may be fixed; it may pass on either side backward or forward. The rectum also makes a curve, and the tip of the coccyx then presses upon the posterior wall of the rectum, and with bipeds, such as we are, we find a mass of feces standing perpendicularly and passing through the canal, the lumen of which is lessened by the point of the coccyx coming forward. If the lumen of the canal has been diminished one-third, the rate of passage must be increased that amount; therefore, we get this irritation upon the tip of the coccyx, and there is where we find most of our epitheliomas or carcinomas.

Within the last three years I have made observations in ten cases of my own and those of my friends, and of this number, there were eight of carcinoma developing over the tip of the coccyx. It developed around the sphincter, then above, and the probabilities are that is the most frequent cause of epithelioma or carcinoma of the rectum.

I have treated three patients in the last fifteen or eighteen years for syphilis. They passed out of my hands, and were operated on for carcinoma of the rectum, and within five months I have known of five cases operated on by competent

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surgeons in Cincinnati that were all syphilitic and are all living today. The suggestion I have to offer is this, that in all cases of supposed carcinoma of the alimentary tract, or carcinoma anywhere, it is our duty to place the patient first upon syphilitic treatment. I do not use salvarsan or neosalvarsan, but rely on mercury in some form given to the limit.

I also find that the glans coccygia is subjected to various malignant changes, and that it has had much to do with the development of carcinoma. That has led me into investigation and research work on the glans coccygia or the so-called coccygeal gland, which is not a gland because it does not possess glandular tissue. We find this body is vestigial in man. It has been passed down to us, and it plays a more important rôle in malignancy than is generally supposed.

I wish to congratulate Dr. Halstead on what he has said and on bringing to us these valuable statistics.

Dr. Leonard Freeman (Denver, Colo.): As I am on the program for the discussion on Dr. Tennant's paper, I shall confine my remarks to what he has said.

His case is an interesting one in every way, and skillfully treated. As I understand the case, it was that of a woman who had cancer of the breast and ulcer of the stomach. Although she had cancer of the breast she did not develop cancer of the stomach, in spite of the fact that she was in the cancer age, that she was manifestly susceptible to cancer, and that ulcer of the stomach is a favorable soil upon which cancer grows. I cannot see, however, although I tried very hard to do so, that this is either unusual or paradoxical. It is the nature of cancer to grow in only one spot, as a primary growth. We have always been taught this. It would be surprising to us, in fact, if cancer did occur in two portions of the body as a primary growth at the same time. I should not be surprised, for instance, and I should not consider it paradoxical, if a woman who had cancer of the breast and a badly lacerated cervix uteri, should fail to have cancer in the lacerated cervix. We should not consider it paradoxical if a man with two ulcers of the stomach should develop cancer in one of these ulcers and not in the other. In fact, I happen to know of one of Dr. Tennant's own cases in which a woman had cancer of the uterus for a long time, and also coincident gall-stones, discovered during the course of a laparotomy. She had cancer of the uterus, but no cancer of the gall-bladder, although gall-stones are well known to predispose to

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cancer. Why these things are, we do not know. Why cancer is, we do not know.

Dr. Tennant has given us an interesting paper dealing with a prominent factor in cancer-growth, but he has not given us any explanation. What we want is an explanation, and when we get it we will have made great progress towards the cure of malignancy.

DR. CHARLES H. MAYO (Rochester, Minn.): Just a word or two as to the paper of Dr. Porter. I believe that all bilateral organs that present diseases are subject to the same stimulus. In the lungs and breast, we get the same type of tumors tending to develop in quite a definite percentage of cases. For instance, in patients under thirty, about seven per cent of all ovarian cysts are malignant; from thirty to the change of life this percentage is doubled, and in old age it is tripled. The average is about fourteen per cent in young and old people. For instance, papilloma of the ovary may become malignant in a certain percentage of cases, and, if you give them time, papillary growths in the urinary bladder will become so, or papillary growths of the rectum which have long continued as definite tumors, so that in the case Dr. Porter mentioned in which an ovary was left, the woman on reaching the change of life is almost certain to develop carcinoma in the remaining ovary. The size makes no particular difference, the small papillomatous cysts may take on malignancy. If the cyst ruptures, the cancer cells are widely grafted. These cysts appear in the forties, or about the change of life, and they look malignant clinically. They may not show definite invasion of cells under the microscope, but in so far as the type of carcinoma cells that are present is concerned, they show the tendency of papillary growths to carcinoma.

If I found a young woman under thirty with one ovary diseased and the other ovary normal, I should leave the normal ovary with the instruction that she should have repeated examinations during the next few years. With carcinoma developing in a woman between thirty and forty-seven, if one ovary shows definite carcinoma, the other ovary should come out; and when carcinoma develops in a woman at forty-seven and upward, both ovaries should surely be removed, including the uterus, and the uterus should come out in any case after both ovaries are removed for carcinoma.

As to the question of carcinoma developing in duodenal

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ulcers: Carcinoma is most common in the stomach, yet with a continuous muscularis and continuous mucosa, it is one of the rarest things we see, not but what it does occur. Carcinoma may come down; and by inoculation, by advance, or by encroachment, it may involve the duodenum. It will metastasize in the liver, the glands, and graft on to the colon, and go on to the pancreas and leave the duodenum alone. Where we have three out of our cases of ulcers in the upper end of the duodenum, we rarely see carcinoma of the duodenum.

This brings up the next point. Where you find carcinoma you will find a mild acid, or a strong acid, area. The strongest acid area is in the stomach, consequently we have the greatest number of carcinomas in the stomach. An absolutely normal saliva is slightly alkaline, but in diseases of the mouth or gums with pyorrhea or cancer there is an acid saliva. In the large bowel we get cancer on account of acidity.

Dr. Daniel N. Eisendrath (Chicago): I want to illustrate my point just a moment by a sketch on the blackboard. I have been interested in the subject of Paget's disease very much, especially during the past year, and the paper of Dr. Tennant brings up a great many points which I believe are just beginning to be appreciated. You doubtless know that there is a great dispute between dermatologists and surgeons, the former claiming that Paget's disease is primarily a lesion of the epidermis of the areola with secondary invasion of the breast, i. e. primary epithelioma of the areola of low-grade malignancy, with secondary invasion of the breast. The majority of surgeons believe that it is a primary duct carcinoma with secondary involvement of the skin by interference with the return lymphatic supply to the areolar tissue or areolar gland around the nipple.

Now, in the case Dr. Tennant has shown us, I am not at all convinced that he has shown in this section that the skin was attacked primarily. The view established abroad is that we are dealing in the majority of these cases with a primary carcinoma arising in the ducts with secondary obstruction of the return supply to the lymphatics from the skin areola, and as a result of that we have a chronic edema of the areola and the invasion of the areola with characteristic Paget's cells—these vacuolated cells.

I will tell you of a recent case of my own. A woman was sent to me with retraction of the nipple, which had existed

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for three months, with absolutely not a bit of induration to be felt. The peculiar nature of the case was that immediately surrounding the nipple there was an area of parchment-like induration of a light yellowish color. Believing in the theory that the primary focus in Paget's disease is a duct carcinoma with secondary involvement of the skin, I made a diagnosis of what we are beginning to appreciate as the first stage of Paget's disease. I believe dermatologists will soon coincide with the view that most of these cases are instances of primary carcinoma of the ducts with secondary involvement of the skin. The breast was removed completely, and we found microscopically and macroscopically a typical condition of primary duct cancer. There was a marked secondary edema of the skin with typical vacuolated cells. The duct carcinoma radiated out in all directions, and was of such a nature that it was impossible to feel it until one took hold of the breast after it was excised because the mass was so deep in the breast.

Dr. Carl B. Davis (Chicago, Ill.): The point Dr. Halstead makes concerning blood in the stools cannot be emphasized too strongly. Some time ago in going over the histories of patients with cancer of the rectum at the Presbyterian Hospital, we found 35 per cent of these people had been operated on once, twice, or three times for hemorrhoids, although cancer was present. Within the last week I have removed the lower bowel in a patient who was given hemorrhoidal ointments in the last three months by two different men. The patient entered the hospital with complete obstruction of the bowels. This point of Dr. Halstead might be dwelt on with considerable vigor.

I think Dr. Halstead's estimate of 66 per cent mortality for the combined operation is hardly modern. These discouraging statistics include an older set of records compiled by men who used the combined, or abdominosacral, operation only as a secondary makeshift. Fifteen years ago the abdominal step was not attempted commonly as a preliminary measure. The surgeon after starting by the sacral or perineal route found the case inoperable, and then with a ruptured bowel or incomplete preparation opened the abdomen. A larger percentage of such cases naturally died of sepsis.

Again, cases inoperable by the Kraske route are bad risks to start with. If we operate on such a patient by the combined

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method, and the patient dies, I do not see why the Kraske route should receive any favor in the statistics.

A third point should be considered in determining whether we should do some posterior form of operation or a combined operation. The worst set of statistics for the combined operation shows a low mortality in the female and a much higher mortality in the male, because of the relation of the prostate, vesicles, and urethra to the rectum. In estimating the chances of a patient by the combined procedure we should give the female the extra advantage.

In addition to immediate mortality we should consider the ultimate mortality or prognosis after operation. Hartman of Paris collected last year several thousand cases of cancer of the rectum treated surgically.

For the various forms of sacral or perineal operation he found an immediate mortality of 15.8 per cent with a recurrence of 68.1 per cent. For the combined operation he found a mortality of 37 per cent, including the statistics of the older operators, and a recurrence of 18 per cent. Thus 37 per cent plus 18 per cent shows an ultimate mortality of 55 per cent in patients operated on by the combined procedure. But 15.8 per cent plus 68.1 per cent shows an ultimate mortality of 83.9 per cent in patients operated on by the single posterior route.

The question of an abdominal or sacral anus is of importance. The abdominal anus, if properly constructed, is more likely to be competent, is far less subject to unpleasant accidents when the bowels are loose, and is under the vision of the patient, and thus is more readily handled and cleansed.

Dr. Herman E. Pearse (Kansas City, Mo.): I would like to speak on the paper on cancer of the rectum, and only on a very limited portion of it. I would like to urge that, in every case in which we have carcinoma of the rectum to deal with, an abdominal incision be made as a preliminary step. I say that, as opposed perhaps to the view of the essayist, there should always be an inguinal anus or colostomy. I say it on account of the experience I have had in the General Charity Hospital with reference to people coming, down and out; and the principal reason for their return is a recurrence at the site of the original growth or higher up in the abdomen, and the reason of their filthy condition is a colostomy. By an abdominal incision made immediately as the first step, we are enabled to find the presence of any metastasis that contraindicates an extensive operation. We cannot find that through

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an incision made for a preliminary colostomy. Having made the abdominal incision we are enabled to make an abdominal anus. Again and again have these patients come to you with an opening so far to the left, so close to the anterior superior spine of the ilium, that a pad could not be worn. A well-placed rubber cap and pad with belt and truss will entirely control the fecal output, and will render these patients capable of appearing in society and of coming in contact daily with the world without dread and without serious discomfort. This cannot be worn on account of the proximity to the anterior superior spine of the ilium if the patient is at all emaciated. The anus must be out in the center of the abdomen. So, it seems to me, all things considered, the preliminary point of attack in every case of cancer of the rectum should be an abdominal incision in the left rectus muscle.

Dr. J. W. Andrews (Mankato, Minn.): I have but a word or two to say on the last case presented by Dr. Porter. I think all who have had a large experience in abdominal surgery in the female will admit that solid tumor of the ovary is very, very rare. I have had a fairly large experience in abdominal and pelvic work, and I do not recall ever having encountered but one large solid tumor of the ovary. This tumor weighed four pounds, and upon section somewhat resembled the tumor presented by Dr. Porter. I thought of its malignancy at the time. I had a section of it examined by Dr. Belfield, of Chicago, and it was found to be purely a fibroid. It had that appearance, and it cut like a fibroid. I do not understand that in Dr. Porter's case he confirmed the diagnosis of malignancy by microscopic examination, but merely by the macroscopic appearance. If I am wrong in that the doctor in summing up will correct me. But with the absence of fluid, with the character of the tumor, in the absence of microscopic examination to confirm the diagnosis, is it not possible that it was a fibroid tumor rather than a malignant one? The case I referred to in my own experience had a very small pedicle. So it is possible to have a large solid tumor of the ovary resembling in its macroscopic appearance a fibroid of the uterus that is not malignant.

Dr. Donald Macrae (Council Bluffs, Iowa): It seems to me that this is an extremely important subject. Just a word in regard to Dr. Tennant's paper. I think the ground has been pretty well covered by the discussion of Dr. Mayo; but

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the discussion of Dr. Eisendrath in relation to the conflict between the dermatologist and the surgeon, and the splitting of hairs as to whether the disease is on top of the skin or an inch below it, is of no particular interest to the practical surgeon. If it is a carcinoma on the skin or half an inch below the skin, or three inches, the point is to get it out before it becomes malignant if you possibly can. While I compliment Dr. Tennant on his case-report, I think in a case of that kind where we have Paget's disease from an ulcer on the nipple, without involvement of the glands in the axilla, I would remove the breast at least. The way to get ahead of cancer of the breast is to remove it early.

With reference to the paper of Dr. Halstead: I have been in this game for twenty-five years myself, and while my experience is limited, yet I believe I have had about the same experience with carcinoma of the rectum that most of you have. I have tried various methods of treatment, from the Kraske up, and I must say that my results are not extremely satisfactory. I suppose most of you have had good results, and while personally my immediate mortality has not been high, my end-results are not very good. I have made up my mind personally on this one thing: *If I cannot spare the anus, I will not operate on carcinoma of the rectum in the future.* I shall not deter a patient from undergoing an operation, but I shall tell him he had better seek the aid of some other surgeon who has better results than I. The horrible condition of the majority of these patients after operation is worse than death in the most cases. While we talk about the end-results, let us talk about the condition of the patient in the interim between the time of his operation and his death, five or six years later, if the surgeon is unlucky enough to pull him through for that length of time. I have seen many patients who have refused operation for carcinoma of the rectum, who have gone on for six or seven years in fairly good health. They have not suffered the horrors of the artificial anus, whether it be by the Kraske or the abdominal operation, and I sometimes question whether or not we should not attempt to standardize an operation for the relief of this condition as we spoke yesterday in the way of first-aid standardization.

I have had a number of cases in which I have made a high Kraske, in which I have resected the rectum, pulled down the sigmoid and attached it to the lower end of the bowel, where

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the anus has not been involved, and I have had remarkable success. Even though they live a few years, their life is comparatively satisfactory. I have been sending away patients in whom the anus must be removed, and I have seen the results in these cases after they have been operated on by other surgeons. They have not been benefited; and the lives they live are far from satisfactory. Personally, I would not care to live that particular kind of life.

These points possibly are not in keeping with the views of the surgeons present, but, it seems to me, they are worthy of serious thought as to whether the results of the local or combined methods warrant us in operating on these cases. While we hear of the statistics of men who have patients living six years after operation, those statistics are from the larger clinics, and thousands and thousands of operations are performed that are not included in those statistics. I do not mean to say I would not relieve a man's pain when he has obstruction of the bowel. There, colostomy comes in to make life as easy as you can for the patient, but many of these cases operated on primarily are not sufferers. Although they have a little blood and pus, and think they have internal hemorrhoids, still they are not suffering except for those symptoms, and in that condition a man can go about his business for months and months and even for several years without great discomfort. Some of the cases diagnosed as cancer of the rectum are not in reality true cancer. While we should rely on the microscope as an aid to diagnosis, it is an instrument that is not infallible. We are confused occasionally. If we find an ulcer, and it feels like cancer, we are apt to conclude it is cancer. If we remove a section of it, and send it to a pathologist for examination he may say it is cancer, but there is always danger of mistaking cancer for tuberculosis or for syphilis, as Dr. Ricketts has pointed out. I have no doubt many of us have resected the rectum for syphilitic disease, believing at the time it was carcinoma, and yet under antisyphilitic treatment these cases have cleared up.

Dr. Wm T. Reynolds (Kansas City, Mo.): The papers have all been very well discussed. There is nothing of interest I could add; however, in connection with Dr. Porter's paper regarding "Malignant Tumors of the Ovary," I would like to make a preliminary report of a case that recently came under my care.

I was forced to operate upon a woman, thirty-two years of

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age, for acute intestinal obstruction with no time for making a refined diagnosis, as the operation was distinctly a life-saving measure. She had a tumor which occupied the entire pelvis with a history of very rapid growth. On opening the abdomen it was found she had double ovarian tumor, each one as large as a full-term child's head. Sections of these tumors proved to be villous carcinoma.

The cause of her obstruction was a circumscribed annular growth at the junction of the descending colon and sigmoid. Sections of this growth proved to be adenocarcinoma. I was discussing this case with Dr. Moore last evening, and he told me they had not met with this type of case in the pathological laboratory of the University of Minnesota. It struck me as being a rather rare thing, but, not having looked up the literature, I do not know. I hope to be able to make a full report of this case later as I am quite sure we shall have an opportunity for post-mortem study.

DR. CHARLES A. L. REED (Cincinnati, Ohio): I want to engage the attention of the Association only for a moment, and because, it seems to me, this is a convenient opportunity to present some preliminary observations that I have made with respect more particularly to a principle involved in the treatment of cancer. What I have to say will possibly be apropos of the subject as presented by my friend Dr. Porter.

Some time ago I had occasion to make some observations on the effect which colloid metals brought in contact with tissues exercised, and I found they exercised a selective destructive action upon tissue of the embryonal type, while they exercised no such influence upon the mature or normal tissue. With this idea in mind I sought to apply the principle to the treatment of cancer. I did this, of course, quite experimentally. I did it in cases of advanced carcinoma, primarily of the uterus and involving the pelvic structures. My method of application consisted in availing myself of the arterial supply to the infected area. In other words, if you are to reach all cancer cells, you must reach them through the medium of distribution that goes to all of them, and that is the arterial supply, and not the venous supply. I felt it was necessary to reach the disease locally, and not to avail myself of the general circulation, in the midst of which any medicament would become so diluted and attenuated as to be readily innocuous when it reached the affected parts. Therefore, in these cases of advanced carcinosis, with a full explanation of

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what was intended, and of what could and what could not be realized, I put these patients upon the table, opened the abdomen by a free incision, exposed the pelvic vessels, isolated the internal iliac, and injected from 10 to 20 cc. of the colloid metals. I used several of these metals in the different cases. I used colloid zinc on one side in the operation, repeated it upon the other side by using colloid copper. I selected these metals in a perfectly empirical way because I might intensify their action by employing electro-opposites.

What happened in these cases? In each of them there was an offensive discharging crater; in each of them there was active hemorrhage. The vault of the vagina, the wall of the rectum, the wall of the bladder, and the broad ligaments were all involved in this discharging and suppurating crater. Within twelve hours all hemorrhage ceased. Within twenty-four hours the offensiveness of the discharge had disappeared. In three cases the granulating surface became converted into a dry eschar, and came away. We only removed a few days later the entire single mass, leaving apparently healthy tissue in the background. The active symptoms disappeared and, to all intents and purposes, these cases which involved directly the suppurating area recovered. But they were cases of diffused carcinosis. The disease was already beyond the area supplied by the internal iliacs. Therefore, all of the cancerous area was not brought under control, and the disease went on in a general and progressive way, minus the local manifestations, to an inevitably fatal issue.

I mention this only as a preliminary of a method which I have tried in seven cases, each of them with results such as I have spoken of.

The question in my mind is whether or not I may have overlooked the primary disease when I know the disease is still within the area that can be controlled by this arterial supply. It seems to me that what I have realized in these cases experimentally justified me in making this effort after a due explanation to these patients. Furthermore, I feel the general unsatisfactoriness of surgical intervention as ordinarily applied to these cases, also the general unsatisfactoriness of the *x*-ray in connection with recurrences, and likewise the general unsatisfactoriness of radium in this particular class of cases; and the question occurs to me whether we are not

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justified in an effort to control the disease by intraarterial injections.

DR. PORTER (closing): I have nothing further to say, except that I do not think there is much danger ordinarily of the capable clinician mistaking a fibroid of the ovary for carcinoma, and, further, that this was pronounced an adenocarcinoma on the basis of the laboratory reports.

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BLEEDING NIPPLES

DEAN LEWIS, A.B., M.D., F.A.C.S.

CHICAGO, ILLINOIS

The clinical significance of a discharge from the nipple of a non-lactating breast has been variously interpreted. Saar, in an article published in 1907, found that a discharge from the nipple occurred frequently with cystadenomata. It was found in 31 per cent (15 out of 48) of the cases analyzed by him. In some instances it was the first symptom which attracted the patient's attention and for which the surgeon was consulted. The amount of secretion varied, usually coming out in drops; but when pressure was exerted it could often be forced out in a stream. It was watery, milky, or serohemorrhagic; rarely pure blood.

A scanty, thin, sanguinolent discharge is usually regarded as suggestive of carcinoma; a mucoid discharge, of a benign growth; and a markedly bloody fluid, as of an intracanalicular papilloma. Bloodgood states that a discharge from the nipple, except during lactation, may be looked upon as a sign of a benign lesion and not as a symptom of cancer. If the discharge is serum or blood, this is a positive sign of an intracanalicular papilloma. In senile parenchymatous hypertrophy one can often express from the nipple a thick brownish material, the accumulation of degenerated epithelium. This the patient rarely observes. In a personal communication Bloodgood also states

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that he is inclined to think that many surgeons look upon a discharge of blood from the nipple as a sign of cancer.

The pathological processes associated with the discharge of a hemorrhagic or serohemorrhagic character from the nipple have many interesting features. The tumor most frequently associated with the discharge is regarded by some (Kaufman) as rare, but most surgeons having considerable material can recall some few cases of bleeding nipples, most of which have never been reported or carefully analyzed. The pathological changes associated with this symptom have been so variously interpreted and have received such a variety of names,—encysted medullary carcinoma, cystofibroma, villous carcinoma, and duct cancers,—that it is often impossible to determine whether the lesion under consideration is benign or malignant. By some,—Koenig, Saase, and Greenough,—the intracanalicular papilloma, by all odds the lesion most frequently associated with a discharge from the nipple, is regarded as distinct from abnormal involution, while Saar and Tietze consider this type of papilloma as intimately associated with or a part of abnormal involution, and consider the two together.

I have observed clinically seven cases of bleeding nipples, five of which have been operated upon. One case had been operated upon previously, and several cysts removed. A discharge from the nipple subsequently developed in this case, but no operation has been performed up to the present time. The following histories indicate the course of the disease associated with the discharge and the pathological findings.

Case 1.—Mrs. W., aged 42. Married, but never pregnant. One sister has had a radical removal of the left breast for carcinoma; lately there has been a recurrence in the scar. Five years ago I operated upon Mrs. W., and removed two

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small cysts from the left breast and one from the right. These cysts, the largest of which was the size of a walnut, contained a thick milky fluid. When examined histologically, the cyst-wall, which was composed of thick fibrous tissue, was found to be lined by a layer of degenerating epithelium, which in some places had almost disappeared. Three years after these cysts were removed a milky discharge from the left nipple was noted. This continued for about one year. One year later a watery secretion was noted from the right nipple. Last January this discharge became serohemorrhagic in character, the amount of blood being increased by manipulation of the breast. During March, April, and June no discharge was noted, and during the past three months there has been none. The milky discharge has gradually grown less in amount, until at the present time there is very little. There has been no distinct tumor or cyst-formation associated with the cessation of the discharge.

The diagnosis of senile parenchymatous hypertrophy was made when the cysts were removed, some small cysts being found adjacent to the larger ones. When I examined the patient last spring a serohemorrhagic discharge from the right nipple was noted, and a somewhat milky discharge from the left. A careful search was made for a tumor beneath the left areola, but none could be found. A secretion could be expressed from the nipple when the breast was manipulated, but no definite tumor could be found, although the breast had the peculiar shotty feel of microcystic diseases. Operation was refused by the patient. Subsequently the discharge stopped, apparently without the filling of a cyst, as might have been expected if a duct containing a papilloma had become occluded. There is no clinical evidence of any malignant change in the breast. An operation is undoubtedly indicated, but I believe that a plastic resection of the breast rather than removal should be attempted.

Case 2 is considered with the preceding because both are apparently examples of abnormal involution associated with a serohemorrhagic discharge from the nipples.

Case 2.—Mrs. A. T., aged 37, was admitted to the Presbyterian Hospital February 11, 1915. She has given birth to three children. It was noticed that the clothing covering the

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nipples was blood-stained in June, 1914. Shortly after this the patient became conscious of an itching sensation about the nipples, but did not know whether there was any relation between this and the bloody discharge. September 7, 1914, she consulted a surgeon who advised that both breasts be removed, a radical operation being advocated. A second physician advised that nothing be done for three months. During September and October the bleeding seemed to be more profuse, and was great enough to show through the clothing. During the past few months the discharge has not been very profuse. There has always been more discharge from the left than right nipple. The itching sensation has been more marked about the right nipple. Both breasts are rather large. When palpated they have the shotty feel of microcystic disease. A serohemorrhagic discharge can be expressed from the nipples when the breasts are palpated. No single cysts beneath the areolæ can be felt.

An operation, consisting of a plastic resection, was performed upon each breast, through a curved incision made on the median side of the areola. The entire glandular substance of the breast was removed through this incision. The glandular tissue was fairly riddled with small cysts, varying in size from a buckshot to a cherry. Some of the cysts had a milky content; others, serohemorrhagic contents. No distinct papillary growths could be seen on gross examination. Because of the peculiar granular feel of some of the cysts a careful histological examination was made of tissue removed from different parts of the gland in order to be sure that there were no malignant changes.

Microscopic examination revealed distinct evidences of papillæ-formation. The epithelium in many of the cysts was grouped to form definite papillary growths supported by a connective-tissue stalk. This is a form of the adenocystic type of chronic mastitis. The same process results in the formation of papillomata in the ducts, the growths most frequently associated with a serohemorrhagic discharge from the nipple.

Case 3.—Mrs. A. D., aged 51. Patient had given birth to one child. Nine months before entering hospital she sustained a slight injury of the right breast. Four months later she noticed a nodule in the breast, located superficially, about one inch above the nipple. During the last three months there has been at times a discharge from the nipple. At first this was serous in character. Later it became tinged with

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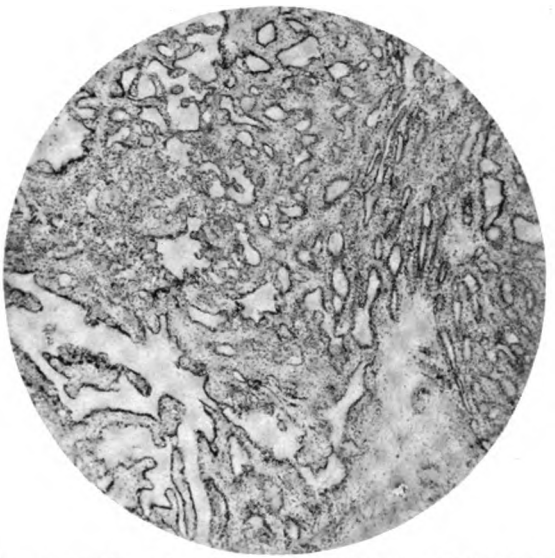

Fig. 1. Section of papillary cystadenoma situated in one of the larger ducts near the nipple. The cyst was the size of a hazelnut. When pressed upon this cyst could be reduced in size, and a stream of a serohemorrhagic fluid, at times of almost pure blood, could be forced from the nipple. There had been an intermittent discharge of a serohemorrhagic fluid from the nipple for eight years. A tumor situated superficially just to the inner side of the nipple developed two weeks before the operation was performed. (See Case 4.)

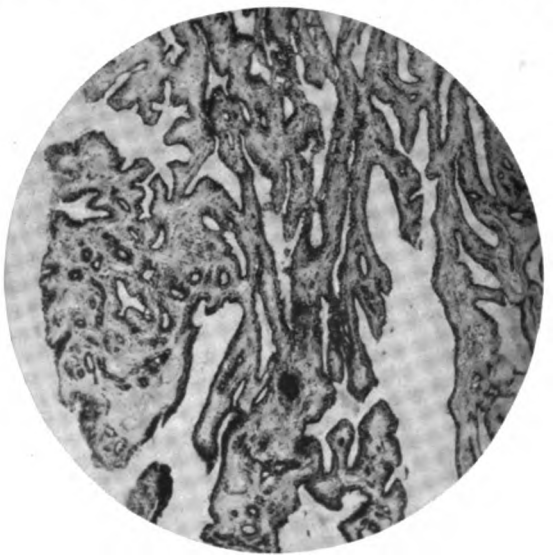

Fig. 2. Section of an intracanalicular cystadenoma removed from a breast in which no tumor could be palpated. A serohemorrhagic discharge from the nipple had been noted for eight months. The papilloma, not larger than a small pea, was situated deeply in one of the ducts. It was so small that it could not be palpated. (See Case 6.)

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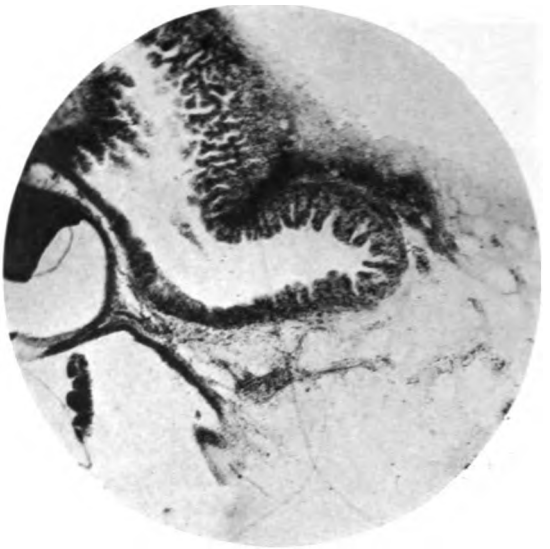

Fig. 3. Section through the acinus of a breast on which a plastic operation was performed. The papillary formation is very marked. The breast presents the typical changes of adenocystic disease. Hemorrhage is associated with papillary growths whether occurring in the acini or ducts (see Case 2). Compare with following section from a breast upon which a plastic operation was performed for abnormal involution in which there was no discharge from the nipple.

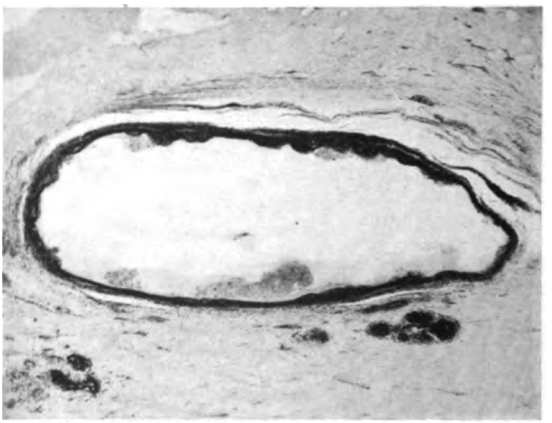

Fig. 4. Section through an acinus of a breast upon which a plastic operation was performed for abnormal involution. The epithelium lining the greater part of this acinus has disappeared. The remaining epithelium shows marked degeneration. The contents of the distended acinus were milky. There was no discharge from the nipple.

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blood. The discharge is intermittent. There may be intervals of ten days or more during which there is no discharge; then the nodule in the breast enlarges, and there is a sensation of fullness. When the discharge is poured out the nodule decreases in size, and the sense of fullness is lost. The nodule is tender on pressure. There is no spontaneous pain.

The nodule is the size of a hazelnut. It is situated beneath the areola above the nipple. When this nodule is pressed upon a serohemorrhagic discharge can be expressed from the nipple. At the time the patient was examined the discharge consisted almost entirely of pure blood, which could be expressed, drop by drop, when the nodule was pressed upon.

A radical operation was performed in this case because of the suspicion of malignancy. When the nodule was incised a distinct intracanalicular papilloma was found. The base of this cyst has a distinctly granular feel and appearance, differing from the intracanalicular papillomata about to be described.

Upon histological examination, it was found that the epithelium had broken through the basement membrane at some points. Malignant degeneration of the papilloma had already occurred, but the operation had evidently been performed at a very early stage of the malignant change.

Within a year a large mass developed rather suddenly in the left breast, which was removed at another clinic. There were no microscopic evidences of malignancy in this breast, which represented the cystic type of chronic mastitis. This patient was operated upon in the spring of 1911 for the malignant papilloma, and there has been no recurrence of the growth.

Case 4.—Mrs. N. W., aged 46, was admitted to the hospital April 13, 1914. For eight years she had noticed an intermittent discharge from the left nipple. The discharge had recurred every four or six months. For nine weeks before entering the hospital the discharge had been almost continuous. Two weeks before entrance she noticed a tumor about the size of a hazelnut, situated superficially beneath the areola to the inner side of the nipple. This tumor developed rather quickly. The general examination revealed nothing, and the remaining history has no bearing upon the subject under discussion.

A serohemorrhagic discharge can be expressed from the

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left nipple when pressure is made upon the small tumor above mentioned. The tumor could be reduced in size by pressure, and the serohemorrhagic discharge became more bloody when pressure was made. The tumor was not adherent to any of the surrounding structures, and could be easily displaced. The examination of the breast was otherwise negative. No cysts or irregularities could be found in the right breast.

A radical operation was performed. Upon section of the tumor, a typical intracanalicular papilloma was found. There were no evidences of malignancy. A gross examination of the other parts of the breast revealed none of the changes associated with abnormal involution and no other papillomata.

The patient has remained well since the operation, and as far as can be learned there have been no changes in the remaining breast.

Figure 1 indicates the histological picture of this typical intracanalicular papilloma. Irregular tortuous openings and spaces are found and there is apparently an attempt to form ducts and acini. Club-shaped processes project into some of the larger spaces. These are covered by epithelium supported upon a connective-tissue stalk, the ends of which in some instances are rather thick. They are so thick in some instances that the villus-like projection might easily break off, and thus given rise to bleeding within the duct. The glandular type of epithelial reproduction is not always preserved, and in some instances there might be a suspicion of carcinoma, for the bases of the villus-like ingrowths are in some places so thin that it is difficult to determine the exact limits existing between epithelium and connective tissue.

The two following cases are examples of bleeding nipples in which no tumor could be palpated, but during the operation a small papilloma was found within a milk duct, deep in breast tissue.

Case 5.—Mrs. C. F., aged 35. Never pregnant. Was admitted to the Presbyterian Hospital May 12, 1914. She had noticed a discharge from the left nipple for some weeks. At times this was serous in character, but often became blood-stained. The discharge varied considerably in amount, and at times almost entirely ceased. There were no other symptoms. The breast was not perceptibly enlarged, and no pain was complained of. Upon palpation no distinct evidences of

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a tumor could be found, and the breast did not have the shotty feel of adenocystic disease. When pressure was made over the upper and outer quadrant of the breast, a serous fluid could be expressed, and the fluid became blood-stained if the pressure was continued.

As no definite tumor could be found, the upper and outer quadrant of the breast was resected, as pressure over this quadrant caused the nipple to discharge. As one of the larger ducts was cut across near the nipple a serohemorrhagic discharge was noted and deep down in this duct was found a small papilloma about the size of a small pea. As far as could be determined by gross examination there were no changes in the rest of the tissue removed, and there has been no recurrence of the discharge since the operation.

Case 6.—Miss M. B., aged 38, was admitted to the Presbyterian Hospital November 16, 1915. She had noticed a discharge from the right nipple for ten months. At times the discharge had been great enough to soil a rather large breast-covering in twenty-four hours. No distinct pain had been noticed. Neither breast was enlarged. No definite tumor could be felt; but the discharge could be increased in amount when pressure was made over the upper and outer quadrant of the right breast. A plastic resection of the breast according to the Warren method was performed. When one of the large ducts was cut across near the nipple a serohemorrhagic discharge escaped. Situated deeply in this duct was a small intracanalicular papilloma (see Fig. 2), such as that described in Case 5. There were some evidences of beginning microcystic disease upon gross examination, but the changes were not nearly so marked as would be inferred from the findings revealed by palpation of the breast.

Case 7.—This case has been offered me by Dr. Carl B. Davis, who has had the patient under observation for some time.

Mrs. C. F., aged 48, noticed last September some bleeding from the left nipple. The discharge was slight, but was enough to stain the underclothing. The bleeding soon stopped, and now there is only an occasional watery discharge. No tumor could be palpated in the breast, and there were none of the changes associated with cystic mastitis. The clinical findings in the breast corresponded to those in Cases 6 and 7, and the probabilities are that in this case the lesion is an intracanalicular papilloma which is so small and deeply

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situated that it cannot be palpated. Owing to the fact that the discharge is only occasionally observed and has become watery, the patient has refused to have any operation performed upon the breast.

These seven cases all presented a typical serohemorrhagic discharge. The discharge in two of the cases at times became almost pure blood. In two cases the discharge was associated with chronic cystic mastitis, while in the remaining five cases small intracanalicular papillary cystadenomata were the cause of the hemorrhage.

Saase, in an article upon cysts and cystic tumors of the mammary gland, states that the papillary cystadenomata are not related to carcinomata, for the adenomatous new-growths do not infiltrate the surrounding tissue, and do not extend beyond the walls of the duct, which becomes dilated to form the wall of the cyst in which the papillary cystadenoma lies. As these growths are frequently situated superficially it is quite possible that the skin covering them might become ruptured or thinned so that the growth might extend externally; then a small mushroom- or cauliflower-like mass is formed from the papillary cystadenoma. This has happened in some cases in which a cyst situated superficially has been incised, and the growth not removed.

The important question regarding these cysts concerns their relation to malignancy. It is difficult to determine how many cases become malignant, for many of the cases, undoubtedly benign, were formerly regarded as duct cancers, and the cases were not followed subsequently, so that it cannot be determined whether recurrences occurred or not after operation. Bowlby and Mastermann report three local recurrences in sixteen cases collected or observed by them, but these were local, and were the result of an incomplete operation, rather than an evidence of malignancy.

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In a personal communication Bloodgood makes the following statements concerning a discharge of blood from the nipple. His cases are divided into the following groups:

Group A: Cases in which women have had discharge of blood from the nipple without the findings of any tumor. This group can be divided into two classes. Class 1. Two cases have been operated upon. The breast was removed in these two cases and a simple papillomatous cyst containing blood found. One case was operated upon fifteen years ago, and the other five years ago. Both patients are well, without recurrences. Class 2. No operation was performed. He has had about five or eight of these cases. In all but one the blood has disappeared, no tumor has developed, and the patient has remained well. In one case a tumor developed three years later, and was removed. It was a cyst with a papilloma, and there has been no recurrence after a period of about two years.

Group B: Discharge of blood from the nipple associated with a papillomatous tumor. All of these cases have been operated upon and in all a papillomatous cyst has been found. The majority have been benign. He thinks that in only one or two cases which were malignant was there a discharge from the nipple. From this experience he finds that discharge from the nipple alone is not an indication for operation.

Greenough and Simmons reported 20 cases of papillary cystadenomata of the breast in 1907. In eleven of these there was a serohemorrhagic or hemorrhagic discharge from the nipple. Three were malignant, but there is no mention made of a discharge in these three cases. There was one local recurrence in this group. The tumor recurred in the same situation from which one had been removed, and had persisted for four years, as another operation had not been attempted.

Rodman regards papillary cystadenomata as an advanced stage of abnormal involution, and states that they can usually be differentiated from cancer in a clinical way, because cystadenomata are nearly always

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situated immediately behind the nipple. The discharge of pure blood, the central location of the cyst, and the age of the patient, usually forty-eight or forty-nine years on an average, will enable one to recognize it. But Rodman believes that these are potentially malignant from their inception, and that a radical operation should be performed. He has found but two exceptions. Of the six papillary cystadenomata that he has operated upon and has records of, four were definitely malignant, two were not. Rodman also reports two cases of bleeding nipple associated with chronic mastitis.

Histological interpretation of the cellular picture presented by a papillary cystadenoma is often difficult. This is indicated, as previously mentioned, by the reports of earlier observers who regarded these as duct and villous carcinomata. If they were carcinomata there have been relatively few recurrences, even after incomplete operations.

The character of the discharge, whether serohemorrhagic, hemorrhagic, or brownish, apparently gives no clue whether malignant changes are occurring. Some of the benign papillomatous growths have been associated with a brownish discharge. In some instances the discharge has lasted as long as nine years, in one case as long as twelve. One of the cases observed by me showed beginning malignant changes. In this case a discharge had been noted for three months, but a tumor had been present for four. The character of either the discharge or of the tumor did not enable me to make a diagnosis of beginning malignancy as they did not differ from those of a benign papillary cystadenoma. The granular wall of the cyst aroused suspicion of malignancy when the cyst was incised.

Bleeding nipples are most frequently associated with

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intracanalicular papillary cystadenomata and the adenocystic type of chronic mastitis. The papillary growths occurring in the acini or ducts are essentially the same, and the papillary cystadenomata should be regarded as a part of abnormal involution, although not necessarily as a late stage. That the papillary cystadenoma may not be single is indicated by Saase's report. In only one of five cases observed by him were there no evidences of changes in the breast, with the exception of the cyst. Even in this case a statement cannot be made concerning the portion of the breast which was left, for only a small part of the breast immediately adjacent to the papillomatous cyst was removed.

A plastic operation should be performed in most of these. It should be performed, unless there are evidences of malignancy. The changes associated with malignant degeneration, I believe, are quite definite, and can be determined by gross appearance when such a cyst is opened. I believe that an operation should be advised even when there is no evidence of a tumor, for in these cases a small intracanalicular papillary cystadenoma will be found deep down in the ducts. The portion of the breast in which the growth lies can be determined by the increase of the discharge when pressure is made.

REFERENCES

Saase, E.: Ueber Cysten und cystische Tumoren der Mamma. Arch. f. klin. Chir., 1897, liv, 1.
Tietze: Ueber das Cystadenoma Mammæ (Schimmelbusch) und seine Beziehungen z. Carcinom der Brustdruse. Deutsche Ztschr. f. Chir., 1900, lvi, 512.
Saar: Ueber Cystadenoma Mammæ und Mastitis Chronica cystica. Arch. f. klin. Chir., 1907, lxxxiv, 223.
Greenough and Simmons: Papillary Cystadenomata of the Breast. Ann. Surg., Phila., 1907, February.
Rodman: Carcinoma of the Breast. Reprinted from Murphy's Clinics, 1915, iv, No. 2.
Bloodgood: The Clinical and Pathological Differential Diagnosis of Diseases of the Female Breast. Am. J. M. Sc., 1908, cxxxv, 157.

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CANCER OF THE LIP

W. W. GRANT, M. D., F. A. C. S.

DENVER, COLORADO

Cancer is common in the lower lip, and infrequent in the upper; it is common to men, infrequent in women, and rarely occurs in the young. Therefore, we must associate it with the occupations and habits of men chiefly.

As to the etiology we are unable to affirm, or to deny, a specific germ. The germ theory is not inconsistent with injury as a predisposing causal agent. Observation and experience teach its intimate and frequent association with trauma. Cancer does not originate in healthy tissue. Weakened, non-resistant tissue, under such favoring conditions as scar tissue, and fissures and ordinary ulcers, whether due to chemical rays or to persistent local irritation, invite it. The lips, mouth, and tongue are always exposed to irritation and infection from a variety of sources,—ragged teeth, the pressure of a pipe stem, and the constant irritation of tobacco. Eliminating the specific ulcers,—and this is not difficult—one should look with suspicion on every persistent ulcer of the lips, whether in the form of a crack or fissure or the ordinary looking superficial ulcer. As in every other part of the economy, early diagnosis is of the utmost importance, for the simple and unanswerable reason that it determines the prognosis. No one is justified in delaying action for serum tests, therapy, or refined laboratory inves-

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tigations, to corroborate or determine the probable diagnosis of cancer of the lip. Sight and touch, with the clinical history, are usually sufficient. A hard ulcer base, without even manifest glandular involvement, is always suspicious and significant.

I know of no department of surgery which taxes more severely the skill, nice sense of proportion, harmony, and delicate technic than plastic work of the mouth and face.

Early radical excision gives the only promise of permanent relief. It is cruel to torture a patient with caustic pastes, and it is inviting disaster to rely on the Röntgen ray, unless as a post-operative prophylactic measure. Both these agencies will cure a certain number, or class, of superficial ulcerations, but both are also dangerous irritants. Glandular involvement is not always early, even in malignant ulceration, and this explains why, in such cases, the cure may be permanent without the removal of glands. The basal cell forms the basis of these cases. The prickle cell is more malignant and rapid in action. It is well established, however, that any operative procedure, short of *complete* extirpation, does more harm than good. In a well-established or extensive ulceration with glandular involvement, the Röntgen ray may eventually induce more rapid extravasation and metastasis, and any local irritant will do the same thing. If the disease is located between the middle and the corner of the mouth, the glands on that side will be first infected, and for a time the infection will be limited to that side. If the ulcer is in the center of the lip, and also as it approaches the center from the side, the submental glands will be affected, in keeping with the distribution of the lymphatic vessels.

The removal of the diseased lip should be the *first* step of the operation, to avoid the danger of soiling

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the clean-cut tissues by the discharge from the ulcerated tissue. The submaxillary glands and lymph nodes are the receptors and distributors of the virus through the lymph vessels. The sooner the diseased mass (the origin of the infection) is removed, the less the danger of distributing the virus by the manipulations incident to the operation. The reasons given by some writers for reversing this order are not tenable. The excision of the diseased lip is done usually in one or two minutes, while the extirpation of the glands is a slow and often tedious process, if thoroughly done.

Safety demands wide excision and as complete removal as possible, of submaxillary glands and lymph nodes, regardless of evidence of their being involved. In the hands of most operators who have devised methods of procedure, the incisions for flap-formation are independent and distinct from those for the removal of the glands. This is unnecessary. The V incision is a tradition; it should have no place in modern surgery, unless in a very small growth in a large mouth, for with its broad base and apex in healthy chin tissue, it inevitably results in a sucker mouth. Besides, the stable chin tissue is necessary and most important for the fixation of mouth-flaps.

The semilunar incision is objectionable, even in small superficial ulcers, for it leaves a depression at the site of the disease, and when half or more of the lip is excised, it, like the cuneiform operation, is not the most satisfactory condition for the making of efficient flaps in the construction of a mouth. No operation which leaves ugly and needless scars is ever desirable, and this is especially true of the face. That operation is best which involves the fewest incisions necessary for complete work, and replaces the diseased lip by soft elastic tissue, resembling the original

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as nearly as possible, with the least disfigurement and the best cosmetic results.

The stationary chin tissue is of great importance as a point of fixation for the sliding flaps which constitute the reconstructed mouth. The chin flaps of Langenbeck, Esmarch, Kowalzig, Blasius, Volkmann, Serre, Celsus, Syme, Buchanan, and Morgan are all objectionable for the reason that this hard inelastic tissue is not suitable, and a further objection is the absence of mucous membrane, which is not conveniently supplied. The Morgan operation, Esmarch, Kowalzig 1829, Regner operation, Binnie 1912, are the same in technic. The chin flaps framed in this way tend to retract and drop downward, with entropion of the lip and dribbling of saliva. The cheek flaps of Bruns, Estlander, Sedilot, Diffenbach, Trillat, and Trendelenburg are needlessly mutilating and disfiguring, and always endanger Stenson's duct. The sliding chin and cheek flaps of Sutton, Nélaton, Ombredanne, and Stewart have no fixed base, and are objectionable for the same reason. In the Morgan operation the divided elliptic space beneath the jaw is closed partly by stitches, grafts, and granulation tissue. These chin flaps do not make a mobile mouth.

I may be pardoned for again expressing the opinion that the best operative procedure for the reconstruction of the mouth involving one or both lips is the one which I devised, and first described[1] in 1899, and again more elaborately[2] in 1905.

AUTHOR'S TECHNIC

The growth is excised by rectangular incisions, the base uniting the two vertical incisions being ordinarily in the depression, or crease, between the apex of the chin and the lip. From the two inferior angles of the wound, incisions are made obliquely downward and outward, dividing all tissues, including the mucous membrane, and crossing the maxillary

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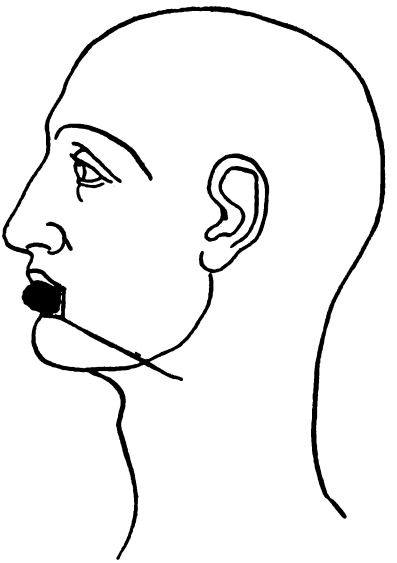

Fig. 1. First step of operation.

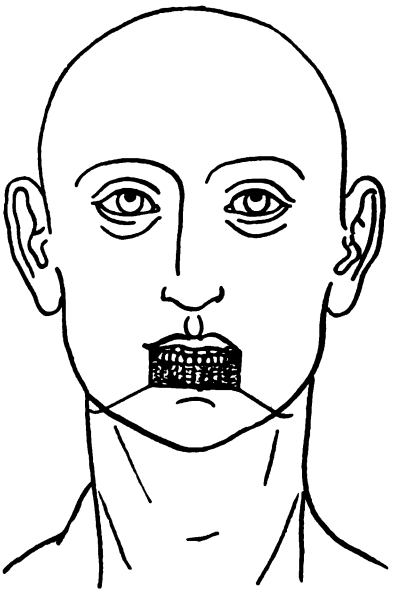

Fig. 2. Second stage of operation.

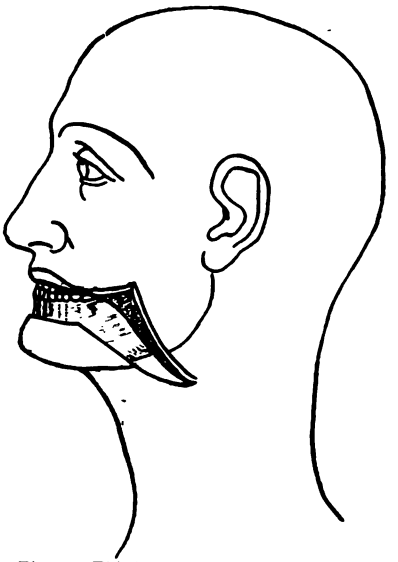

Fig. 3. Third stage of operation—removal of glands.

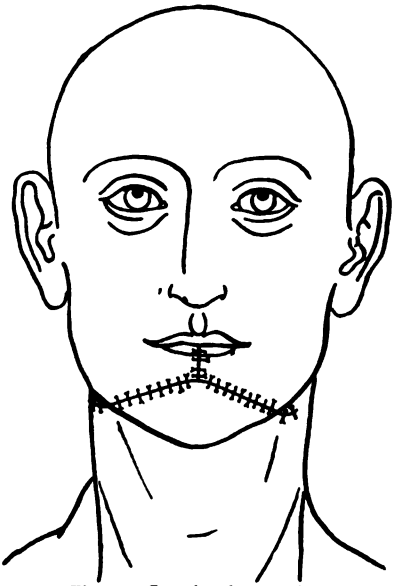

Fig. 4. Completed operation.

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Fig. 5. For widening mouth. First step: mucous membrane ready for dividing in middle line.

Fig. 6. Completed operation for widening the mouth.

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Fig. 7. Width of lip excised seven-eighths of an inch. In tumor or disfigurement the oblique incisions, upward and outward, with triangular flaps, detached from maxilla, will restore the lip to normal proportions without wrinkling of the skin or short upper lip.

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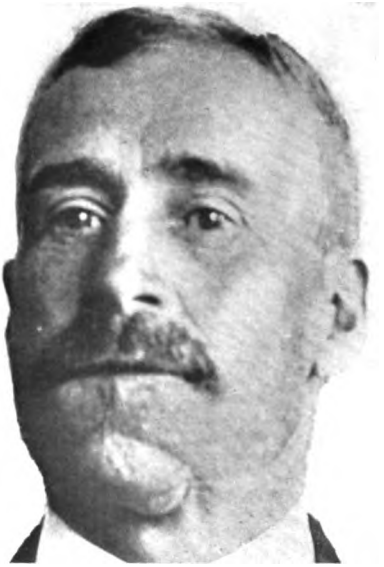

Fig. 8. Excision of three-fourths of the lip for cancer. Mouth enlarged as illustrated in Figs. 5 and 6.

Fig. 9. Carcinoma of the lip. Duration three years. Primary x-ray treatment and subsequent cautery. Half of the lip excised, and all glands removed, October 4, 1915. Pathology, basal-cell cancer. Mouth not widened by operation at corners.

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line at a point about equidistant between the angle of the maxilla and the symphysis, and then carried backward beneath the maxilla as far as may be necessary to permit the easy removal of glands, and at the same time afford good flaps for the mouth. Stenson's duct is never cut. The operation is repeated on the opposite side, and all glands are cleaned out, the submental, by a vertical incision between the anterior belly of the digastric muscles.

The cheeks are then separated from the maxilla, at least as far as the masseter muscles, if necessary. The facial arteries are cut, but the parts are vascular and the blood supply ample. The flaps are then brought forward and first united to each other with silkworm gut, in the middle line. If the flaps should be too broad to fill the space accurately, they can be retrenched from the inferior border. If there is much tension, a mattress suture is inserted, and tied over gauze. This removes undue tension from the middle suture line.

Stitches of silkworm gut or a running stitch of chromic gut are next applied to the laterial incisions. They should include the entire thickness of the cheek. If half or more of the lip is removed and the mouth does not now admit, without stretching, three fingers of the hand flat, then the mouth is enlarged by an incision backward and slightly downward, from each angle of the mouth, usually from one-half to three-fourths inch in length, down *to* the buccal mucous membrane, and the latter separated from the overlying tissues above and below the incision to the extent of half an inch. The lip above and below is now beveled from the inner border of the incision in order to facilitate easy covering of the lips by the mucous membrane, which is *now* divided with scissors in the middle line, nearly to the angle of the skin incision, and united to the skin by a running stitch of chromic gut. (Fig. 5 and 6.)

When it is necessary, from the extent of lip removed, to employ this additional procedure, the levator anguli oris muscles are divided, and, in consequence, the upper lip will tilt slightly for two or three months. While this is not a serious matter, being opposed by the depressors, still I am in the habit of cutting away a small wedge of skin and muscle from the upper side of the wound, with the apex at the angle of the mouth, in order to limit this temporary inconvenience. Further to control the action of these and the

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superioris muscles during the healing process, I apply a strip of adhesive plaster from cheek to cheek across the upper lip. The levators may be put out of action temporarily by subcutaneous division.

A small rubber drain is inserted from the posterior angle of each lateral incision beneath the maxilla, and a T drain through the submental incision into the mouth, under the tip of the tongue. The T end rests behind the incisor teeth, and the outer end is fixed by a safety-pin. This drain is especially important, and limits the soiling of the wounds. Without it, saliva and mouth and throat secretions would constantly bathe the lip and wounds, and might interrupt prompt and satisfactory healing.

Though the upper lip is seldom affected by cancer, the same principle and operative technic will apply that has proved so satisfactory in the lower lip. Rectangular incisions for removal of the growth, and oblique incisions upward and outward for the formation of flaps, will satisfactorily meet the indications and necessities of the case. If the triangular shaped flaps thus formed are united while being held in downward traction, and at the same time stitched to the fixed tissue beneath the maxillary spine, the short upper lip frequently observed will not result, and this procedure, I may add, will prevent the short, concave lip so common after the ordinary harelip operation.

In the ordinary operation, all glands can be removed and the flaps perfected through the same incisions. The new lip tissue is the most suitable for the purpose, thick, soft, and elastic, presenting a good appearance and mobile lips. In the lower lip there is not a cicatrix above the angle of the mouth, and there is not a denuded surface in the whole operation to be healed by skin grafting or granulation. The cosmetic results are the best attainable. If the growth is distinctly to one side of the mouth, it follows that the flap from the opposite side will be longer than the one from the diseased side. It makes no difference, therefore, whether or not the flaps meet in the middle line. Covering of flaps and vermilion border with mucous

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membrane is essential to prevent disfiguring contraction.

POSTOPERATIVE TREATMENT

No condition probably requires more attentive and skilful after-treatment. The first dressing should be changed in from twenty-four to thirty-six hours, and repeated at these intervals for a week. At such time, the mouth should be irrigated through the T tube as well as through the mouth, with warm boric acid and normal salt solution. The patient should be nourished with liquids through a feeding-tube until the stitches are removed after a week or ten days.

I emphasize some of these details because the mouth is never aseptic (if at all, but a short time) under *any* conditions, and in cancer is never clean.

Post-operative treatment often determines the measure of our success in many conditions, and in none does it produce more satisfactory results than in cheiloplastic work of mouth and face.

REFERENCES

1. Grant, W. W.: Med. Rec., New York, March, 1899.
2. Grant, W. W.: Operation for Cancer of the Mouth. Jour. Am. Med. Assn., Sept. 30, 1905, p. 962.

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NOTES ON THE ETIOLOGY AND PROPHYLAXIS OF CANCER

HENRY T. BYFORD, M. D., F. A. C. S.

CHICAGO, ILLINOIS

While the form in which this communication is cast resembles that of an argument, it is not expected to prove anything. My aim is to present a few fairly well established facts that have led me to draw certain conclusions with regard to the etiology, and, from these conclusions, to formulate such recommendations of a prophylactic nature as they may seem to justify.

1. Carcinoma is most prevalent between the ages of forty and seventy years. It is seldom observed in children, and less and less often after the seventieth year.

2. In men, carcinoma affects the stomach most often, and the intestines next. In women, it affects the neck of the uterus most often, the breast next, and the stomach next.

3. Primary carcinoma does not attack healthy tissues, nor is it a disease of old age, although age may diminish resistance to it. It has some relation to a weakened local resistance of epithelial structures, due partly to changes incident to chronic irritation and partly to changes connected with a decline or impairment of function in these structures. We have an illustration of such chronic irritation in carcinoma of the lips and stomach, and of decline or impairment

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of function, or deterioration of structure, in carcinoma of the breast and ovary, and of both in carcinoma of the uterus. Women bearing children frequently are not so often affected as those who have not conceived for a long time. The same is true of mice. (Dr. Maud Slye.)

4. Occupation has a decided influence upon its prevalence. Chimney-sweeps, hotel servants, industrial laborers, brewers, furriers, general laborers in cities, and traveling agents are among those with a high mortality, while coal-miners, farmers, teachers, clergymen, agricultural laborers, locksmiths, gas-fitters, tanners, pressmen, compositors, and printers are at the other end of the list.

5. Carcinoma is increasing in frequency, both in this country and in Europe. The increase is chiefly in men, and is greater in the alimentary canal than elsewhere. The coincident increase of gastric ulcer probably has something to do with it. A study of the Paris death-reports for the years 1886 to 1905 seems to show that carcinoma of the uterus and carinoma of the breast were practically the same during that period, while carcinoma of the stomach increased from 21 to 46 per thousand, and carcinoma of the intestine from 9 to 15. Cabot and others have demonstrated that a large proportion of internal diseases are diagnosed erroneously. It is not improbable that many deaths from carcinoma have not been diagnosed correctly in the past, and that, with greater frequency of autopsies and a better knowledge of internal medicine, more cases are now recognized than formerly. Carcinoma of the skin, which has probably always been diagnosed, has not shown any increase.

6. Carcinoma is more prevalent in populous districts, both urban and rural, than in newly settled areas,

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more among civilized people than savages, more in human beings than in domestic animals, more in domestic than in wild animals. Among domestic animals it is found most in cats, dogs, and mice, which eat man's food and dirt, and least in sheep, which associate less intimately with man and do not eat his food. Dogs and cats have more stomach and bowel affections than sheep. The dog has his attacks of nausea, diarrhea, and constipation, while the cat is often seen to vomit and have fits.

Carcinoma occurs with relative frequency in people in prosperous surroundings, who either travel or associate with travelers, and who eat foods brought from different places. Sailors have a comparatively high mortality, while fishermen in out-of-the-way fishing villages often show a very low mortality. Furriers, who work with skins with the hair and dirt on them, have a high rate of mortality, while tanners, who work with skins that have been through processes that would remove and kill germs, have a comparatively low rate.

7. Carcinoma is to a certain extent a disease of locality. It is much more prevalent in the temperate zone than in the torrid and frigid zones, more in low and damp districts than high and dry ones. In localities quite near one another the incidence varies greatly according to local favoring conditions. For example Bela (Centralbl. f. Bakteriol., 1898, vol. xxiv) gives the following facts about Luckau:

"Its eastern suburb, which is low-lying and marshy, being intersected by irrigation channels for market gardening, comprising 1,000 inhabitants, furnished 73 cancer deaths, or one in nine of its total mortality. * * * The cancer mortality of this suburb was four-fold that of the central part of Luckau.

"In curious contrast with the foregoing, the high-

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lying and dry suburb of Sando, comprising 1,000 inhabitants, had not a single death from cancer during the whole twenty-two and one-half years."

Many similar observations are on record.

Gardeners have a rather low rate of mortality, yet one observer found that gardeners about Berlin had a high rate. This would seem to indicate that in this case some local influence was the cause.

8. Cancer hardly ever affects the pancreas or duodenal ulcers, and but seldom the liver and spleen. From these facts, and others that are too well known to require special mention, I have drawn the following conclusions:

1. We are justified in assuming that carcinoma is an infection, and that it will not be a waste of time to make a review of facts and probabilities on this basis.

It is difficult in any other way to account for the extent of its unusual prevalence in one of the suburbs of Luckau and among the gardeners of Berlin, or for a death-rate of 9.5 per thousand in Sweden against 0.41 in Brazil, or for its selection of chronic sores and non-functioning glands as sites of invasion, or for the local effect of trypsin in cleaning off, and keeping clean, the ulcerated surfaces of invaded tissue. It is also difficult to account in any other way for the greater relative increase of carcinoma in populous districts, both urban and rural, than elsewhere. In such districts there is more traffic and travel, and thus greater opportunities for the introduction and spread of infection.

The fact that a fresh wound in a healthy individual cannot be inoculated, while patients with carcinoma sometimes have secondary growths produced in fresh wounds, indicates that there is a certain kind of immunity to the infection that such patients have lost.

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It is easy to conceive how a cancer germ may find lodgment in an area of chronic inflammatory action where the circulation is impaired and the epithelial structure has deteriorated, and may resist the handicapped immunizing process of the patient until it has established its own immunity. This is, in a way, the opposite of inoculating a patient possessing ordinary immunizing powers with an attenuated virus, in order to establish his or her immunity to them. The necessity of overcoming the patient's immunity may mean a long incubation period, and make it difficult to trace the infection to its source. There is nothing strange about the impossibility of primary inoculation since it is also a characteristic of the bacillus lepræ.

When a primary carcinoma is removed, and a few slightly infected glands are overlooked, the patient may recover the normal immunity, and it may take the few remaining germs anywhere from one to several years to overcome it. Or the immunizing process may have been stimulated to increased activity by the primary growth, and may be able to cope with the few that remain.

2. Although carcinoma is sometimes inoculated into the skin or other external epithelial surface it is in a great preponderance of cases introduced into the system with the food.

Its comparatively frequent primary occurrence in the alimentary canal, from the mouth to the rectum, is convincing evidence of this. The frequent primary invasion of the breast, uterus, and gall-bladder in women can hardly be explained by its entrance in any other way. The liability of ordinary food to contamination by handling and exposure is shown by the following quotation from the *Journal of the American Medical Association,* vol. lxv, No. 23, p. 2015:

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"Prof. C. H. La Wall, chemist of the Pennsylvania Dairy and Food Commission, is said to have found the following assortment of objects and substances in raisins exposed for sale on a Philadelphia street: pieces of prunes, beans, and rice; strands of human hair and cat fur; cotton and wool fiber; straw and bits of bran; insect wings and legs; cigar and cigaret ashes; and a yellowed cigaret paper."

3. The human feces are carriers of germs of carcinoma both in individuals affected with the disease and in many who are not. The same may be said about the feces of the dog and the cat.

The occurrence of primary infection in the colon and upper rectum shows that the germs that get by the pancreatic secretions can survive to infect the rectum. If they reach the rectum alive they can of course be passed out and may find lodgment elsewhere. The comparative frequency of rectal cancer in dogs would make the same reasoning apply to them. The fact that dogs seldom have carcinoma of the bowels above the rectum would indicate that they are comparatively free from chronic lesions of those parts. The same can of course occur in human beings who have no susceptible areas in the alimentary canal. Such infections of the feces can be made to explain how chimney-sweeps, who are frequently affected with chronic irritation of the scrotum and are probably not always over-cleanly in their habits, thus get the scrotum inoculated. Their occupation provides the lesion for inoculation. Infection of the feces can also be made to account for the frequency of carcinoma of the cervix uteri. Unless a woman washes the parts thoroughly, or takes a sitz bath, after each bowel movement, fecal infective matter will not infrequently be left on the perineum about the anus and in the over-

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hanging vulval hair. As most women who have a chronic cervical lesion either examine themselves by indagation, or use douches, tampons, or suppositories, or wear a pessary, or take local treatment, there are plenty of chances for such infection, in addition, of course, to that of coitus. We often find that the colon bacillus has spread to the vulva, urethra, and vagina in these ways. In some cases the germ may be introduced into the vagina without having gone through the individual's own alimentary canal, but in view of the comparative infrequency of carcinoma of the skin I should say that such cases must be rare.

Infected feces are liable to contaminate the soil in manufacturing districts if the sewage is imperfect and the fecal matter is not properly disposed of. This is particularly true in such neighborhoods as those in which mechanics live. Vegetable gardeners and small farmers near large centers have been known to use human excrement as manure.

4. Those who are most subject to carcinoma are those who work the most in dirt and eat the greatest variety of food. Thus chimney-sweeps, industrial laborers in large towns, city laborers, furriers, and carpenters, all of whom have a high rate of mortality, work in dirt and have not always the means or the incentive for frequent washing; while pressmen, compositors, and printers, whose working materials are protected from outside contamination, and whose surroundings are such that they can and do wash and clean up when they go to lunch and go home from work have a low rate. We can account for the fact that coachmen, city-hotel keepers, and hotel servants, and brewers have a high mortality by their food. They eat of food of great variety which has come from different sources, and is liable to be contaminated. They

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are for the same reason liable to have lesions in the alimentary canal that render them susceptible.

Miners whose work, although dirty, is almost entirely below the surface contamination, whose food is simple and more or less sterilized by canning, and who, in general, live in small towns where they are not brought into contact with the great variety of people and infections, have a low death-rate from carcinoma.

5. There are probably several factors that have some influence upon the increase of cancer in recent years.

The increase of railroad traffic may be supposed to have some effect in spreading infection through the travel of individuals and through the enormous amount of cold-storage food that is carried everywhere. Some of the travelers and some of the food must be infected.

The crowding of people in large towns, particularly in industrial centers, has been too rapid and too great for hygienic methods to prevent contagion and contamination, except in those infections that are understood. A general recognition of the infectious nature of carcinoma would probably soon result in a decrease of the mortality, as has recently been the case in tuberculosis and typhoid fever.

The increase has also probably some relation to the increase of ulcer of the stomach, gall-stones, mucous colitis, appendicitis, and chronic intestinal diseases. These affections have become more frequent as a result of the increasing complexity and richness of diet during the past century. Pastries, candies, cakes, sweetmeats, fancy bakery food, nuts, unripe bananas, sweet potatoes, meat of all kinds, etc., are now within the reach of all but the very poorest classes, who do not have cancer as often as those in good circum-

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stances. A hundred years ago sugar was scarce and expensive, and pastry, cake, ice-cream, soda-water, candy, and most of our sweet bakery stuff were either not invented or were too expensive to be used, except in the smallest quantities. But nowadays children begin to ruin their stomachs with such foods, both at meal-time and between meals, from a tender age; and those who can afford it are apt to keep it up until they acquire gastric or intestinal pathology, and are put upon a diet.

A quotation from the article on sugar in the Encyclopedia Britannica will show the reasonableness of this assertion: "While in 1700 the amount [of sugar] used in Great Britain was 10,000 tons, in 1800 it had risen to 150,000 tons, and in 1885 the total quantity used was almost 1,000,000 tons." I suppose that Great Britain's stomach had to handle most of this. This was prior to 1885. A few statistical facts by V. L. Price, of the National Confectioners' Association, will enable us to realize how the consumption of sugar has kept pace in recent years with ulcer and carcinoma of the stomach: "The per capita consumption of candy in the United States is approximately thirty pounds per annum; that means 2,700,000,000 pounds of sweets are sold each year in the United States. There are factories which specialize in the manufacture of marshmallows, of high-boiled candies, such as stick candies, of lozenges, of sugar-coated confections, and of chocolates, and some of the factories in their specialties have outputs of 40,000 to 50,000 pounds a day. Other factories make general lines, and many of them produce as much as 100,000 pounds of candy a day. There are also thousands of smaller factories scattered throughout the United States producing from 500 to 10,000 pounds a day." (V. L.

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Price, "Candy Making an Art," in the Chicago *Evening Post,* Saturday, Sept. 3, 1911.)

While this growing complexity of food and abuse of sweets may not be the cause of ulcer it may have some relation to its increase in frequency.

The fact that the very poor who are crowded together do not always have as high mortality from carcinoma may be in part accounted for by the fact that their food, excluding that which is canned and therefore sterilized, is simple, and does not so often produce susceptible foci as that of the more prosperous, and, being simple and not brought from many distant sources, it is less liable to be contaminated with the carcinoma germ.

The fact that carcinoma has been observed to affect the obese and well-fed and gouty individuals more often than thin people may be interpreted in the same way. The experiments of Dr. Maud Slye upon mice would seem to favor this view as to the greater susceptibility of the well-fed individual.

W. Roger Williams has shown the "mortality to be twice as great among well-to-do men having no specific occupation as among occupied males in general." (Natural History of Cancer, p. 96.)

"Aschoff has shown that in the Berlin population cancer was of the most frequent occurrence in persons of independent means, living on their income or pension." (Williams, l.c. p. 354.)

Thus an increased local susceptibility, due to modern conditions of life and the tendency of any infectious disease, the character of which is not understood, to spread, seems to me sufficient to account for the increase after we have deducted the apparent increase due to former erroneous diagnoses.

6. Since duodenal ulcer is a more common lesion

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than gastric ulcer, and yet seldom becomes infected with carcinoma, and since trypsin which is poured into the duodenum also prevents continued superficial development of carcinoma on surfaces with which it is kept in contact, the question arises whether trypsin, or possibly some vegetable ferment or synthetic imitation, could not be used for the destruction of the disease or the production of immunity. Whether injections of trypsin or a similar substance into and around the carcinomatous mass, or into the afferent blood-vessels, or into the colon or the general circulation, could be worked out so as to be curative, is perhaps worthy of serious thought, if not experiment. The well-known work of Beard is significant, although it was used to support a theory, and is not conclusive.

7. Dr. Maud Slye has shown that immunity and increased susceptibility may be inherited, and that in mice some individuals always develop carcinoma when the local conditions favorable for its development are produced, while others never do. This points to the prevalence of the infective agent and would seem to explain why its presence in the food will successfully inoculate a chronic gastric ulcer in some patients and not in others. The probability is that most of us take it into our stomachs, although but few of us are infected. A healthy alimentary canal is one of the best safeguards against carcinoma excepting, of course, a natural immunity.

The foregoing conclusions have suggested the following recommendations:

1. Carcinoma should be considered an infectious disease.

2. Precautions against the spread of the infection should be taken by the community, as well as by the individuals affected.

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3. Foods, particularly fruits and vegetables, should be protected from contamination at their source and in transit. All fruits and vegetables from general sources should be sterilized before being eaten, except such as have a complete covering or hard external surface that can, after a thorough cleansing, be removed. The use of human excrement as a fertilizer should be prohibited by law.

4. The disposal of human excrement in suburban and populous rural and manufacturing districts should be such as to avoid possible contamination of the surface soil. The feces of patients with carcinoma of the alimentary canal and pelvic organs should receive the same attention as those of patients with typhoid fever or cholera.

Women should be taught the infectious nature of normal stools with particular reference to keeping the perineum free from contamination. They should also be taught to spend a considerable portion of their time washing their hands.

5. The number of cats and dogs in populous districts should be restricted, and they should not be allowed to roam about the streets, by day or night. The excess should be killed. Means should be taken for the extermination of rats, mice, cockroaches, and other vermin. This should have been done long ago as a protection against the spread of other infections.

6. Individuals whose occupations are known to expose them to great risk of infection from carcinoma should be taught that it may get into their system either through the irritated skin or by way of the alimentary canal. They should be taught to keep the skin free from chronic lesions, and should wash their hands very thoroughly before eating, and also to wash and disinfect their hands very thoroughly and change

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their working garments when they leave their work for the day. All workers in dirt should observe these rules.

7. That all epithelial areas affected with chronic irritation and erosion should be attended to, has already been emphasized by others. An attempt might also be made to prevent infection of ulcerated and eroded surfaces in the alimentary canal. Patients with such lesions should, of course, avoid all unsterilized food that might be contaminated. Whether it would be useful to give a large dose of trypsin or something with a similar catabolic or germicidal action once or twice daily in cases of ulcer of the stomach, or a pint or more of a solution of acetozone or alphozone, or some other non-poisonous substance that might be found to have a destructive action upon carcinomatous cells and parasites, is a question I think that deserves consideration.

8. Municipal authorities should put carcinoma upon the list of diseases to be reported in order that the patients may be traced and taught how to take care of themselves and their infected discharges, and that none of those living with them be allowed to handle food stuffs for the market.

9. The blood of patients with carcinoma should be exhaustively studied with reference to the discovery of something that will increase immunity, for, until we learn more about the germ, the establishment of immunity would seem to afford the best hope of preventing infections and recurrences

10. The time seems to be ripe for teaching the public something concerning the erroneous notions about diet that are prevalent among the idle rich and the prosperous poor, in order that they may stop manufacturing the serious forms of gastro-intestinal dis-

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ease that have of late years shown such an alarming increase in frequency, the seeds of which are sown in adolescence and the fruits of which are harvested at maturity and in senescence. In this way the danger of infection by contamination might be greatly diminished.

11. Women who have not borne children for several years should be warned of the danger of developing carcinoma, and should not only be on the lookout for symptoms, but should submit to a pelvic examination at least twice a year until it is evident that the mucous membranes are healthy and are remaining so.

In conclusion, I confess that I may have made more deductions and recommendations than the facts justify, yet I believe it is better to err in overstatement as long as I am not recommending anything that can seriously inconvenience those for whose benefit they are made. My greatest hope is, that what I have said may suggest something that may be of help to those whose work is being pursued along more scientific lines.

FOR DISCUSSION SEE PAGE 253

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THE RELATION OF THE SKIN-TRANSPLANT TO CARCINOMA, ESPECIALLY TO ITS RECURRENCE

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The relation of the skin-transplant to the recurrence of carcinoma has some vital points with respect to the use of these transplants following the removal of a large portion of skin particularly in breast-amputations. The problem involves the following questions:

First. Is a carcinoma more or less liable to recur after skin-grafting? If so, where?—in the skin-graft or outside the skin-graft?

Second. If skin-grafting is done, what form of skin-graft shall be used; that is, shall we use the Tiersch graft, the Reverdin or the Wolffe-Krause graft? Having decided upon the kind of graft to be used, are there any special principles of skin-transplantation preferable to laying these grafts on the area from which the cancer has been removed.

Fourth. When shall the graft be laid?—at the time of operation or later? I will attempt to answer these questions in the following cases, and in order to get the matter fairly before us, I will give a short history of the case which stimulated my interest in this matter.

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CASE

Mr. M., a Bohemian, aged 42, a patient of Dr. Koutsky, of South Omaha, Neb., was admitted to the Wise Memorial Hospital of Omaha on June 3, 1915. His family and previous history was negative, so far as any point that bears on the question under consideration is concerned.

The patient was seen by me first on June 4, 1915, where I elicited the following history of his disease: Four months previous to his entry to the hospital, Dr. Koutsky had removed a V-shaped piece from the left side of his lower lip, on account of a chronic ulcer of the lip, which the physician believed to be cancer, and which, upon microscopic examination, proved to be such. At the time I saw the patient there was a large tumor about the size of a small orange in the submaxillary region just about the position of the submaxillary salivary gland. This tumor was not fluctuating, very hard, affecting the floor of the mouth, and was excruciatingly tender and painful. The patient came to me on account of pain. The *x*-ray showed a slight irregularity in the periosteum of the jaw in this region.

I made a clinical diagnosis of recurrence of carcinoma of the lip on the left side to the right side in the region of the submaxillary gland. The patient would not accept the clinical evidence that this was a cancer, and so I was forced to remove a small portion of the tumor, and the section was made by Professor Schultz, of the College of Medicine of the University of Nebraska, who reported as follows:

> "The first specimen, received June 5, 1915, consisted of a small piece of tissue removed for diagnosis. It is made up of an alveoli composed of large, rather flattened cells of the epidermal type. The alveoli vary greatly in size and are irregular in outline. All but two of the alveoli present in the section have undergone complete keratohyaline change. The two alveoli which have not undergone such change are composed of large, polyhedral cells, whose nuclei are round or oval and whose chromatin content is variable. The small amount of stroma present between the alveoli stains poorly and appears degenerated. Diagnosis: epithelioma."

Instead of the patient submitting at once to the operation as he had agreed, he decided to leave the hospital, and did not return until June 20, there being a period of twelve days be-

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tween the diagnosis and the operation, all of which time the patient complained of excruciating pain on the right side of the face; and the tumor had now become rather large, extending down below submaxillary bone and from about the exit of the mental foramen to the angle of the jaw on the right side, and was as large as an orange. At this time he desired that the tumor be removed by radical operation; and this was done on June 22. We endeavored in the operation to remove all the glands on the right side and as far as the clavicle and down to the third layer of the fascia of the neck, removing the sternocleidomastoid muscle and most of the superficial muscles of the neck, ligating the external carotid artery at its bifurcation, and working upwards to the mouth, removing a portion of geniohyoglossus muscle, the right side of the floor of the mouth, a portion of the tongue, and all of the right side of the jaw from the angle to the symphysis.

I closed this large defect in the side of the face the best we could. The healing took place rather slowly. The lower angle of the wound closed fairly well, as did the floor of the mouth. There was, however, a large cleft in the upper angle of the neck which did not heal as it should, but finally left a rather large defect in the skin below the jaw and in the upper part of the neck. Swallowing was fairly good. I decided finally to skin-graft and attempt to fill the defect. The question arose as to what sort of skin-graft should be used. After some consideration, I chose a Wolffe-Krause graft for the following reasons, discarding a plastic graft on account of the large amount of scar-tissue:

1. The Wolffe graft leaves an elastic area with very little contraction if it heals, which in this region was a valuable asset for cosmetic reasons.

2. The Wolffe graft would build up the cavity and remove a certain per cent of the deformity following this operation.

3. I could hold such a graft in place on the overhanging surface by means of sutures. This would be more difficult with a Tiersch or Reverdin graft.

4. I believed a thick skin-graft would prevent, to some extent, the return of the carcinoma.

In these points the subsequent history proved our reasoning faulty.

5. If it did not heal, a Tiersch or Reverdin graft could be used later.

On July 8, a spindle-shaped transplant of the Wolffe-Krause

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variety was taken from the anterior surface of the thigh, and laid in the wound of the neck. I was careful to make the area where the graft was to be laid as dry as possible, and to protect the transplant from the fluids of the mouth which dribbled over the lip. This was done by means of constant suction for the first twenty-four hours. The transplant was sewed to the edges of the wound by means of Pagenstecher's linen. The transplant seemed to heal rapidly in the sides of the wound and to become attached at its base to the granulating surface.

On July 22 the patient was discharged from the hospital with the granulatory surface in the neck closed.

In October the patient appeared again with considerable swelling and edema over the area of the flap, and therefore I thought the flap was undergoing what frequently happens in full depth transplants,—that is, sloughing of the superficial layers of the skin. I noticed an ulcerating area on the upper side of the flap and about a half inch from the outer edge. This I believed to be a sloughing process. This process continued in spite of all kinds of treatment,—antiseptic dressings, etc. For my own information, I clipped from the sloughing area a portion of the necrotic tissue, and sent it to the laboratory. To my surprise the following report was returned:

> "The specimen received on October 18, 1915, consisted of several bits of tissue, concerning which nothing could be determined upon gross examination. Upon microscopic examination these bits of tissue are shown to be composed almost wholly of tumor alveoli like those of the original tumor. Necrosis is marked. There has been considerable hemorrhage into the necrotic material, and keratohyaline change is present in most of the alveoli which are not necrotic. Diagnosis: epithelioma."

Imagine my surprise at this report when I had supposed such a condition improbable. The cancer rapidly extended until at the present time the patient is moribund.

In the search of the literature for similar cases, I was disappointed in finding only a few references. Upon inquiry among my friends, all seemed to agree that the recurrence of carcinoma in a skin-graft is at least a common thing, but no one seemed to have in mind any particular case. Dr. Bloodgood, in a per-

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sonal communication, advised me that at Johns Hopkins they frequently see the recurrence of carcinoma in skin-grafts for breast-amputations, while Professor Halsted in the *Journal of the American Medical Association* for February 7, 1913, writes as follows:

"Skin-grafts present a definite obstacle to the dissemination of carcinomatous metastases. I have seen cutaneous recurrences along the margins of the graft, spreading with great rapidity in the skin extend to the opposite breast, to the back and over the abdomen to the pelvis without involving the grafted area except by encroachment at its edges."

Professor Halsted here refers entirely to either the Tiersch or the Reverdin grafts. The questions which arose in my mind were the following:

1. Did this extension occur by continuity of tissue or by metastasis through blood-vessels extending from the area operated on into the graft? The swelling seemed to begin on the surface of the graft just beneath the epithelium, which epithelium became edematous, broke down, and ulcerated. In about two weeks the transplant was edematous over a large area in its center, rather than around the periphery which reminded one of a late slough in Wolffe grafts. At about this time swelling and hardness appeared in the skin distant from the graft. Had the microscope not revealed the nature of the ulceration in the graft, a diagnosis of some low grade of infection or the ordinary slough of the skin graft would have been made.

2. Did this carcinoma grow up from beneath the center of the graft by cellular continuity? This might have been so, although I believe not because the pathological condition appeared first on or near the surface of the graft.

3. It is evident that the carcinoma did not extend by continuity of its edges.

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4. The sutures used in sewing in the graft might have carried carcinomatous cells into the graft.

5. The graft may not have been well nourished in its center, and so formed a point of least resistance for the extension of a latent carcinoma from the tissues below.

6. The cancer cells may have been implanted on the surface of the transplant, and a partial epithelial slough have given the cells an opportunity to grow. In an experience of about fifty cases of skin-transplantation this is the only case in which cancer recurred, so far as I know. It is thus the exception rather than the rule. I may answer my first querie as follows:

1. Cancers are usually less liable to occur in skin-grafts than in their adjacent tissue.

2. All skin-grafting done upon a bed from which cancer has been removed, should be very small grafts, like the Reverdin graft, or thin grafts, like the Tiersch, for the following reasons:

a. The progress of the healing area is not covered, and can be kept under observation, and the slightest recurrence attacked as seems best.

b. Thin grafts do not carry with them many arteries and lymphatics, and are thus not so susceptible to recurrence.

c. They less rarely slough, and when they do the thin epithelial cells seem to inhibit the extension of cancer into them.

d. No matter what form of skin-grafts is used, they should not be sewed to the area in which they are transplanted, because sutures carry cells with them.

The discussions of this question show that all surgeons agree that the best time to skin-graft is at the

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time of operation, because at this time no granulations have formed over the area and the grafts are more readily held in place, and the Tiersch grafts are most acceptable. If it becomes necessary to graft later, Reverdin grafts are best, because they can be forced deeper into the denuded granulations, and have more consistency in themselves.

CONCLUSIONS

Skin-grafts on the surface of wounds in which carcinoma has been detected should be thin grafts, because they carry no blood-vessels or lymphatics with them, and for the further reason that they do not completely cover the surface of the wound.

The Tiersch graft should be used on fresh surfaces, and the Reverdin graft on granulated surfaces.

The recurrence of carcinoma in skin-grafts is comparatively rare and these grafts seem to exert a special influence against the return of carcinoma.

DISCUSSION ON THE FOUR PRECEDING PAPERS

DR. B. B. DAVIS (Omaha, Neb.): I have been asked to open the discussion on Dr. Stokes' paper, after which I may have a few words to say about some of the other papers.

Dr. Stokes' case with reference to the skin-graft, which became carcinomatous, appeals to me as being a case, not of metastasis occurring at the point from which he got the graft, but merely as a case of carcinoma growing up through from the base. As a change of sentiment with reference to the skin, I wish to bring up a point in regard to the breast. I do not think it is the custom now to remove such a large amount of skin in cancer of the breast as we formerly did. Although we see recurrence in the skin nearly always, it has been pretty definitely proven that in practically all of these cases the carcinoma grows from subcutaneous fascia, and involves the skin secondarily. It is not logical to remove so much of the skin over large areas as was formerly done. In Dr. Stokes' case we have an instance in which carcinoma grew

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up from underneath, and involved the skin, as it might involve any skin which had not been transplanted.

Just a word or two in regard to the paper of Dr. Grant: I take issue with him in regard to the method of doing the operation in beginning with the lip. If he removes the lip first before removing the submaxillary glands, it seems to me that he should, in the first place, be extremely careful as to whether he squeezes cells into the lymphatics, and should also change his gloves and get new instruments before he attacks the part underneath. The logical method in all these cases of cancer, where we have glands to remove, is to begin at the point farthest away from the diseased area and remove the lymphatics, and make our dissection from that point toward the primary disease. In that way we avoid squeezing cells out into tissue that is not already involved.

I desire to express my appreciation of Dr. Bloodgood's remarks. It seems to me every time I hear him talk, or every time I read one of his papers or articles on the subject of cancer, it does me a lot of good. It stimulates me, and I think all of us are being converted, to a great extent, by Dr. Bloodgood's teachings, from pessimists in regard to a lot of things to optimists; and it does seem to me that his reference to the more benign cases we get now, as compared with the number formerly, is something we can all appreciate.

A short time ago I was speaking to some physicians present at an operation for cancer, and I told them I was positive we were getting more benign growths in the breast now proportionately than we formerly did. I know very positively in my cases I get a very much less proportion of advanced or of late cases than I formerly did, not only in the breast, but in other parts of the body.

I had a woman come to me two or three weeks ago with a lump in the breast. I asked her when she discovered it, and she said, "this morning when I was dressing." It struck me that that is the attitude of mind we want to get the public into in this campaign of education with regard to cancer, teaching them it is a curable disease if removed while still local. The educational propaganda at the present time has reached the physicians of the country to a great extent, and I know a great many practitioners who are sending in cases early, or as soon as they are discovered, instead of watching them to see what is going to develop. Along these lines we will obtain benefit in the treatment of cancer.

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DR. EMIL G. BECK (Chicago, Ill.): I hesitate to discuss this subject because I bring here some work which is not my own, but I have the privilege of showing it. I may not be able to answer all questions because I am not a biologist, and I do not want to get myself into trouble because there are some questions which are unsettled. I refer to the experimental work done in the Research Laboratory at the North Chicago Hospital.

I show you a culture which was obtained by Dr. Buchsbaum from cancer tissue taken from patients in the operating-room under aseptic precautions. The tissue was taken not only from one case, but from seven different cases, some of it from the uterus, and also from secondary growths, where the cancerous nodes were removed far from the originally infected area. I also show a slide here which is from this culture, which is a pure one, and you can examine it here under the microscope. Where the light is good you can see the characteristics of it. The culture material looks different in different cases—that is, the color varies from this pale-reddish color up to a deep-red. The culture resists certain fluids, that is, certain fluids do not kill it. Dr. Buchsbaum, who is a biologist, placed this culture in *absolute alcohol* and made subcultures from this alcoholic solution. It was also immersed in a *5 per cent hydrochloric acid* solution and it grew as well as before. It has resisted *1 per cent formalin* and *.5 per cent carbolic acid.* Growth has not been retarded by *weak solutions of sodium and potassium hydrate.* Furthermore, the cultures have been exposed to *the x-ray in large doses,* and this has not retarded the growth. It has been exposed to *10 milligrams of radium element* for forty-eight hours, and this has not retarded the growth.

These cultures have been used for experimental purposes in small animals. Mice, two guinea-pigs, pigeons, and one monkey have been used. In the monkey, which is still alive, we have seen no evidence of disease. We have inoculated mice, and one out of every five developed tumors.

I here show you some pictures of these growths which were taken stereoscopically. The tumors grow sometimes at the site of injection and sometimes in distant parts, such as the liver, lung, breast, and genitalia.

I also show here a preserved specimen. We have taken Lumier pictures which show these tumors as they appear in the original color. For several weeks we did not notice that

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there was any change in the animals until they came to autopsy, when tumors were found in different organs. Sometimes the tumors grew in such large proportions that the animal could hardly walk. Another characteristic thing was that many of these mice were blind.

The microscopic examination of the growths in some cases is characteristic of cancer, and has been submitted to many pathologists in Chicago for examination; and at a meeting of the Chicago Pathological Society, where this subject was presented, one of the pathologists present (Dr. Wells) pronounced it a true cancer.

From these tumors which were removed from the mice cultures were again obtained the same as you see here, and even from sub-cultures we could obtain the growth. Again, from the secondary cultures taken out of tumors, new tumors could be reproduced in one of the mice. From the cultures of this tumor we have obtained again the same kind of growth.

We do not claim that this solves the problem of the origin of cancer. There are many things that are not explained.

Dr. Buchsbaum has made a study of the classification of the organism but he has not made it public, so that I cannot speak about that phase of the subject.

A culture from the tumor does not grow like microbes grow. It takes ten or twelve days before anything appears in the tube which is very important.

DR. EASTMAN: What is the medium?

DR. BECK: Ordinary culture medium.

DR. MILES F. PORTER (Fort Wayne, Ind.): I also wish to thank Dr. Bloodgood for his talk. If I understood him correctly, he said that with large cysts with distinct walls we could excise these cysts, and be morally certain we were not dealing with cancer. Is that right, Dr. Bloodgood?

DR. BLOODGOOD: I said large, smooth-walled cysts, not containing papilloma or grumous material or blood, but containing yellow material or slightly brown material with smooth walls. In my experience, embracing some fifty cases where we have only excised the cyst, there has been but one cancer in twenty-five years, and the cancer occurred in another portion of the breast less than three years afterwards, and was recognized by the patient who had been operated on. It is the only one we have.

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DR. PORTER: Would you call smoky fluid grumous fluid?

DR. BLOODGOOD: Smoky is not grumous. Grumous material would be like oatmeal fluid, with breaking down of the cancer. I have never seen it smoky.

Another characteristic of these cysts is, that, if you cut down upon the cyst slowly and check hemorrhage, you will see a blue dome. Perhaps there may be some one in the audience who will tell you that this blue dome is characteristic of malignancy, the same as the strawberry gall-bladder. If you have not seen it, it is because you have not cut down slowly enough. It is like the strawberry gall-bladder, and you may not see it because you may not come from a strawberry section; but I believe there is a strawberry gall-bladder, and I am not depending upon my own color-sense. I have convinced my associates of everything I have told you. They have seen this blue dome. My own students can recognize this blue dome. This blue dome, in my own mind, with apologies to Dr. Mayo, is more easily recognized than the strawberry gall-bladder.

DR. PORTER: The case of tumor of the breast I referred to this morning was just exactly such a case as Dr. Bloodgood has described as being non-malignant. The fluid was what I should call smoky; it was distinctly not grumous. One cyst was distinct from the other, and I believed so thoroughly in what Dr. Bloodgood said that I felt confident it was non-malignant. The operation was continued. When I got my report from the laboratory it stated, "It does not look good to me." I had serial sections made subsequently, and in reporting upon them the pathologist made the report I gave this morning. This is a very interesting case because the microscopic evidences of malignancy are out of proportion to the macroscopic.

DR. BLOODGOOD: How long ago did you operate?

DR. PORTER: It is just a recent case. I made a radical operation. I have the whole breast, and will be glad to send it to you.

Again, Dr. Bloodgood referred to abscess-formation. Several years ago I was consulted by a woman who about three months prior to that was nursing a baby. At this time her physician had found a well-defined cyst in her breast, opened it, and introduced a drainage tube. There followed infection.

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She came to me after a few weeks with the sinus open, with granulation tissue about it, and with a condition that I thought was malignant appearing in the abscess, which had developed as a result of tapping of the cyst. She died of an acute secondary carcinoma of the lung within a few months after the radical removal of that breast.

I want to make one point in this connection, and it is this, emphasizing what Dr. Bloodgood has said, namely, I feel myself that if I saw a tumor which appeared to me as malignant, I do not care what the microscopist says, I would remove it. If, on the other hand, I am in doubt about it, and the microscopist sends in a positive report, I feel it my duty to act upon it, and remove the tumor.

About these leaking breasts: There are a great many cases of cancer of the breast without any tumor at all, and the cases of this type I have seen have been leaking breasts. Some of the breasts that leak blood without tumor-formation are non-malignant, while others of them are malignant. Some of the breasts that leak this sticky, straw-colored fluid are malignant, while others are not malignant. I cannot go into the details at this time, but I have absolute proof of these statements, and I do not know how we are going to make any more conservative operation on these breasts and be sure we are right than by removing the whole mammary gland. Personally, I would not be satisfied with anything less than the removal of the whole mammary gland. When you remove a thing that is non-malignant, well and good, your patient will live and be happy, comparatively speaking, while, on the other hand, if you make a mistake, you do not find it out until after the funeral.

Dr. Leonard Freeman (Denver, Colo.): I wish to speak, first, of Dr. Grant's paper. I have done his operation almost as long as Dr. Grant himself, having adopted it after he first described it to me. I have done many cases by this operation, and I want to say that I consider it a most satisfactory operation for cancer of the lip when the cancer is of some size and requires the formation of a new lip.

In regard to Dr. Stokes' paper: You will remember he placed a full-thickness skin-graft upon a denuded area in the neck from which he had removed cancer, and in that full-thickness skin-graft there developed cancer; therefore, he argues that the full-thickness skin-grafts are not as good as the Thiersch grafts or the Reverdin grafts for such a purpose.

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I have had some experience along this line which has led me to exactly the opposite conclusion. There are certain cases of *x*-ray burns that present themselves as warty growths and closely resemble epitheliomata. Many of them are malignant, and some are on the border-line. In such a case very extensive skin-grafting by the Thiersch method had been resorted to by another surgeon. All of the grafts broke down, the warty growths forcing up through the skin-grafts until they were all destroyed. I then replaced the Thiersch grafts by full-thickness grafts, and had an opportunity of observing the results for some time. Not one of these grafts became diseased. I did not use any sutures in connection with these grafts, which may have contributed to the success of the procedure.

DR. JABEZ N. JACKSON (Kansas City, Mo.): I wish, first, to say a word in commendation of Dr. Grant's operation. We have used his method exclusively now for several years and in a fairly large number of cases, and believe it to be in every respect the most satisfactory method with which we are familiar. It is, indeed, a pretty piece of surgery, if the operator understands plastic surgery, and can do it correctly. When we have an extensive epithelioma involving practically the entire lower lip, one has to take out the entire lip and reconstruct a new one for it. It is marvlous to see what a good result is obtained by this method, and how nearly normal the patient's appearance may be made.

In my own work I am accustomed to make the lateral incision, however, just a little differently from that shown in the diagram of Dr. Grant. I do not carry it quite so obliquely laterally, but come down more nearly vertically, following the natural curve of the prominence of the chin in the line of facial expression. I think this makes it rather easier when the flap beneath the level of the jaw is undermined to reach particularly the submental group of glands toward the midline.

Where an extensive removal of the lip has been made and it is necessary to enlarge the lower lip by a lateral incision, I found one criticism in my original work in that I had correspondingly made a wider upper lip than was necessary, so that it hung over the lower lip and was attended by an annoying amount of leakage of mucus.

I now run an incision upward from the normal corner of

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the upper lip, turning slightly or following the natural line of expression of the corner of the mouth, and take out a wedge-shape portion of tissue from the cheek with its base corresponding to the full width of the lateral incision intended for enlarging the lower lip. When this wound is brought together, therefore, the upper lip is just exactly its original width, and probably the lower has been made just about the same. While the scar from this wedge excision falls in the natural groove at the angle of the mouth, and is practically not noticeable at all.

Concerning these tumors of the breast: There are a certain number of cases in which there presents a definite large cyst, which leads the operator to believe that he is dealing with simply a localized cyst tumor. This is excised by a local operation, and we may overlook the fact that this breast may be riddled with multiple cysts scattered all through it, and that properly the entire breast at least should have been removed. Such a condition of the breast, while usually benign, is, nevertheless, potentially malignant, and should have been amputated. At this time a simple amputation without extensive glandular or muscular removal would have been adequate.

I have had, personally, one sad experience along this line. Some years ago I operated upon a case of this sort, and made a rather wide excision of what I thought to be a single cyst. It was examined by a frozen section and, later, by a finished section, and pronounced benign. Five years later there appeared a new tumor, and the case when operated upon the second time by another surgeon presented a classical carcinoma.

I do not believe that this case was malignant at the time, or, at least, in the cyst excised, but somewhere else in the breast was either an overlooked carcinoma or more probably a cyst which later developed malignancy. We should, therefore, be sure that the cyst which attracts our attention is truly a solitary or focal cyst; otherwise it is safer to do a simple amputation.

Dr. Dean Lewis (Chicago, Ill.): Regarding Dr. Porter's statement that these cysts are essentially malignant tumors: I think he is right. We have statistics to show that of all cases operated on, sixteen per cent are malignant. I do not think there is much trouble about recognizing epitheliomatous cysts, whether malignant or not, because a malignant cyst will

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have a peculiar granular feeling at the base of the papilla, which is characteristic. After the cyst is opened one may make a macroscopic diagnosis of malignancy, and if he makes that diagnosis he should resect the breast, take it all off, and do a radical operation. If we get patients with these cysts earlier, we should have more satisfactory results. Of course, in benign lesions we would do a less radical operation. In these cases of bleeding nipple the patients do not want to undergo a radical operation if the breast can be preserved without mutilation. But in cystic disease the breast should be taken off, and whenever I see these small grape-like tumors with large cysts I take out all gland tissue, and do a plastic operation, which can be done and preserve the breast, and, if histologic examination shows it is necessary to do a radical operation, I do not hesitate to do it.

In one case of large cyst of the breast on which I operated I was rather suspicious about the nature of the disease, and had serial sections made, and it proved to be a small scirrhous carcinoma. In this case I used the cautery, and the patient is still alive. It is now three years since the operation was done.

Dr. Grant (closing): In reply to the suggestion made by Dr. Davis: I assume that every experienced surgeon takes all the precautions of which he speaks, in reference to the soiling of gloves and hands, and thus infecting the tissues with them. If there is an ulceration of the lip I invariably cauterize it with carbolic acid and never touch the lip even with the fingered glove, but with sterile gauze. With a sharp knife you can remove the lip in a minute. It seems strange to me that surgeons do not realize that they cannot with impunity squeeze and manipulate the already infected lymphatic glands and do no mischief, with lip and tumor intact, seeming oblivious of the fact that this stage of the operation may take from one to two hours. There is more danger of spreading the infection by removing the glands first and leaving the excision of the focus of infection for the last.

In reference to the remarks made by Dr. Jackson: I will say that those who may not have done the operation may not know that the defect of which he speaks can be remedied, as I have suggested, without cutting the levator muscle by a perpendicular incision.

In reference to cystic disease of the breast of which Dr.

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Jackson has spoken: I make it a rule in all cases in recent years not to operate on a breast without the presence of a pathologist, who makes a frozen section in ten minutes' time at least. This is, I think, advisable, because it gives, not only the surgeon, but the patient, the benefit of any doubt that may exist as to the diagnosis. In chronic cystic mastitis I remove the entire breast, but do not touch the glands.

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EXCISION VERSUS GASTRO-ENTEROSTOMY

VAN BUREN KNOTT, M.D.

SIOUX CITY, IOWA

Surgeons of experience have long since recognized the value of gastro-enterostomy in the treatment of gastric and duodenal ulcer. When the indications for its performance are present and when made by competent hands the results following this operation have justified the confidence with which it has been so frequently employed. It must be admitted, however, that, like many other surgical procedures that have been popularized by competent men, it has been too frequently employed when sufficient indications for such employment did not exist.

It is a well-established fact that a gastro-enterostomy made in the absence of well-defined and well-understood pathological conditions involving the stomach or duodenum, or both, will prove worse than disappointing. Yet far too many operations are being performed by surgeons who open the abdomen expecting to find an ulcer, and who, failing to find one, can think of nothing else to do than the gastro-enterostomy, which they had decided one or two days before, was the procedure necessary in that particular instance. The value of any surgical procedure cannot be determined by men who are not qualified or by the recital of experiences in which such procedure was clearly contraindicated. Therefore, we may assume that it has been the abuse of gastro-enterostomy rather

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than its proper and rational application that has brought it more or less into disfavor.

It is with full appreciation of the value of gastro-enterostomy when properly employed that I protest against its employment in every case of ulcer treated surgically. Statistics now available from many large clinics prove most conclusively that in approximately 70 per cent of patients afflicted with cancer of the stomach it is implanted upon an old ulcer base. It is recognized that the tissue-changes which accompany chronic gastric or duodenal ulcer are more or less permanent and potentially cancerous.

Rodman, recognizing this years ago, recommended that, where the pyloric region was the seat of an ulcer or ulcers which presented much thickening or other evidences of malignancy, a pylorectomy be performed; and this principle for ulcers so located has been more or less generally recognized and employed. It would appear, however, from a perusal of the literature pertaining to gastric surgery, and from impressions gained in attendance at various clinics, that ulcers situated in other regions of the stomach than the pylorus are not excised as frequently as they should be. With the tendency to the development of cancer upon an ulcer base, regardless of its location in the stomach-wall, before us, the duty of the surgeon toward these patients is clear.

Ulcers so situated as to permit excision without particularly increasing the immediate risk of the operation should be excised. Moynihan[1] states that in reviewing his early work, when gastro-enterostomy alone was employed in the surgical treatment of ulcer, only about one-third of the patients could be pronounced "cured." Another third, after a varying period of freedom from symptoms, had to submit to a

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second operation in which a pylorectomy or excision of the ulcer was rendered necessary, after which they experienced lasting relief. The remaining third developed carcinoma of the stomach from two to five years later. His experience, which parallels that of many other surgeons, furnishes additional reason for the more radical treatment of ulcer at the primary operation. The fact that one-third of the patients with gastric ulcer who have been treated by gastro-enterostomy alone by a master of gastric surgery, require a second operation for the removal of the ulcer before obtaining complete relief from distressing symptoms, should command our earnest attention.

Because of the rich blood supply and the thickness of the stomach-walls, the repair of a wound therein is rendered, not only extremely easy of accomplishment, but practically certain as to prompt and firm union.

Ulcers situated upon the anterior surface of the stomach and not involving the curvatures may be very readily excised, and should be so treated in practically every instance.

Ulcers located in the pyloric region upon the anterior wall may, as a rule, be readily excised; and should such excision result in narrowing of the pylorus, a gastro-enterostomy should also be made. If several ulcers are present in this region or if the thickening and induration attending one ulcer are extreme, a pylorectomy or a Rodman operation should be done. Pylorectomy is a somewhat more formidable procedure than simple excision or excision and gastro-enterostomy, and should be reserved for those cases where the pyloric involvement is quite general and extensive.

Ulcers involving the lesser curvature may be excised by the method of Moynihan,[2] followed by gastro-enterostomy, which, he states, should always be done

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in such cases following excision because of the resulting stomach-deformity. It is surprising what results may be secured by this method in ulcers involving so much tissue on either side of the curvature as would appear at first sight to render their complete removal out of the question; however, many ulcers so situated will be encountered whose complete excision is out of the question because of adhesions and extensive involvement of the stomach-wall, and in such instances we are forced to be content with gastro-enterostomy.

Ulcers upon the posterior wall of the stomach may be excised if not too adherent, either by employing the rotation method of Summers[3] or the transgastric method advocated by Chaput,[4] Deaver,[5] and others. I have employed both of these procedures with much satisfaction, and have been surprised and gratified with the ease of their performance in suitable cases. Because of adhesion or inaccessibility, not all ulcers of the posterior wall can be excised, and a gastro-enterostomy must suffice.

Ulcers high in the cardiac end of the stomach are not amenable to excision, nor are they benefited by gastro-enterostomy. Jejunostomy offers the best prospect for relief to patients so afflicted.

When operating for perforation of a gastric or duodenal ulcer, excision of the tissue involved in the ulcerative process should be done whenever possible before attempting to close the rent in the stomach-wall. This removal of diseased tissue not only facilitates immediate wound repair, but also lessens the tendency to recurrence.

Whenever possible after excision of an ulcer or ulcers the suture line should be re-enforced by an omental graft or flap as was long ago recommended

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by Senn, for thereby the danger of leakage is reduced to the minimum.

As experience increases it will be found that more and more ulcer-bearing tissue may be treated radically by excision even to the point of making, when necessary, a partial gastrectomy for ulcers so situated as to require the same for their removal.

Gastro-enterostomy may frequently be necessary as a supplementary procedure to excision of ulcers wherever situated, and should, in such instances, never be neglected. Believing that the ulcer of today is the cancer of tomorrow, it is urged that in every suitable case the ulcer-bearing tissue be removed by whatever means or method is in that particular instance most safe and practical.

BIBLIOGRAPHY

1. Moynihan: Abdominal Operations, vol. 1, p. 259.
2. Ibid, p. 263.
3. Summers, J. E.: Jour. of the A. M. A., vol. 56, p. 1699.
4. Chaput: Bull. et Mem. Soc. de Chir., 1894, p. 462.
5. Deaver: Jour. of the Am. Med. Sciences, vol. 149, p. 325.

DISCUSSION

DR. LEONARD FREEMAN (Denver, Colo.): We excise ulcers of the stomach for two purposes,—for the purpose of getting rid of the ulcer, and for the purpose of preventing the formation of cancer. I am not going to say anything about getting rid of the ulcer, because the time is too short. The most interesting phase of the question is the prevention of the development of cancer. We all admit that cancer is engrafted upon ulcer in the majority of cases. That does not mean that most ulcers produce cancer, but that most cancers come from ulcers. If this is true, we should always excise every ulcer of the stomach, provided the danger of excision is no greater than the danger of subsequent formation of cancer. But that depends upon so many factors relating to the operator and the patient that it is difficult to formulate a general rule, and each case has to be considered by itself. A society of this kind, it seems to me, has a considerable responsibility to meet, as it stands in a position where its dictum may be followed by a great many different surgeons; hence that dictum should be carefully given. It should be for the aver-

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age surgeon, and not for the specialist, the expert. The expert can do many things that the average surgeon would not dare to attempt, or, if he did, he might do considerable damage.

Looking at the question from this point of view, and not from the side of the specialist, it would seem to me that some such rule as this could be rather safely formulated: If we leave every ulcer and refuse to excise any ulcer at all, a certain number of cancers will result. If we excise every ulcer, a certain increase in the mortality will result. Hence it is the duty of the average surgeon to try to strike such a balance as will be in favor of the patient. Such a balance, of course, implies a considerable knowledge of pathology,—as to the kind of ulcers that are apt to become cancer and where they are apt to exist. But I think we can say that it is possible for most of us to roughly divide ulcers of the stomach into those that are suspicious of cancer as we see them upon the operating-table and those which are not suspicious of cancer as we see them upon the operating-table. All those that are suspicious of cancer should be excised, if possible; all those that we consider not suspicious of cancer should also be excised, *provided it can be done without much danger*. If it can only be done with danger, recognizing the limitations of the operator, then we should content ourselves with a gastro-enterostomy. To be sure, if we do this, a certain number of cancers will be left, but they probably would be comparatively few, and, I think, most of us are willing to say that the increased mortality due to universal excision would be greater than the number of overlooked cancers. In making our decision, of course, we have to consider the fact that cancers are seldom found in the duodenum and comparatively seldom in the body of the stomach, their habitat being the pyloric region, and hence with that region we have to deal. I think the mortality would be high if the average surgeon were to follow the dictum of certain experts, like Dr. Rodman, who advocate always complete excision of the pyloric region.

Dr. M. L. Harris (Chicago, Ill.): With what the essayist has said regarding the performance of gastro-enterostomy in these cases in which it is not indicated, I fully agree. I think we must go very slowly in adopting at the present time any general rule covering the question of excision in preference to gastro-enterostomy. We need more data regarding

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those cases of ulcers which are cured by gastro-enterostomy and those which are not benefited by the operation. There are certain cases which we know pretty well now are not benefited by gastro-enterostomy. Cases of callous ulcers, tumor-formation ulcers, and those ulcers remote from the pylorus and pyloric region, are not likely to be benefited by simple gastro-enterostomy, and therefore they require some other line of treatment. Resection of the ulcer-bearing area of the stomach seems to be the line of treatment best adapted to these cases. However, we must remember that a very large percentage of the cases of ulcer in the region of the pylorus are cured permanently by simple gastro-enterostomy.

The ulcers that become cancerous need further investigation. We do not know yet which ulcers will likely become cancerous. That cancers have their origin on the bases of ulcers seems likely, but that all ulcers of the stomach become cancerous is far from being the fact. In a recent publication, taking some of the actual figures, Dr. William J. Mayo publishes 234 gastro-enterostomies for actually demonstrated ulcers. These were all traced to the ultimate results. Only 14, or 6 per cent, had died, and of these only 2 had died from any stomach condition. Ninety per cent were permanently cured or greatly improved. If, in suitably selected cases, only 2 per cent eventually die of any condition connected with the stomach, it certainly means that cancer rarely occurs on the base of ulcers which have been cured by gastro-enterostomy. That is a very different statement from that made by Moynihan, as given by the essayist, that one-third of these cases eventually have carcinoma of the stomach. Therefore, when we have sufficient knowledge we must discriminate between those cases which are likely to become carcinomatous and which are not benefited by gastro-enterostomy and those which are likely to be cured by simple gastro-enterostomy. We should do simple gastro-enterostomy in those cases which experience proves are likely to recover permanently, and not have a subsequent carcinoma, and reserve excision and resection for those cases which experience shows us are not benefited by gastro-enterostomy, or which are likely to become carcinomatous.

Dr. L. L. McArthur (Chicago, Ill.): I have a little message to convey along these lines which is recalled by a final remark that Dr. Knott made in speaking of the use of an omental flap as covering the line of suture when possible after

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resection of the ulcer. It is a transmission from former ages of the dogma probably originating with Senn,—namely, after making a gastro-enterostomy, at suturing the transverse mesocolon by three or four or five stitches to the line of suture. Dr. Mayo has emphasized the desirability of including the intestine and stomach and posterior mesocolon as omental grafts along that line of suture; and, following the traditions, I have done so.

Ten days ago I had this most unfortunate experience, though I was cognizant of the danger that might occur by not suturing well this omental graft for two reasons: first, Moschkowitz, New York, had recently called attention to seven cases, one of which he had had, and one of which Dr. William J. Mayo had had, in which insufficiency of the sutured graft had permitted the small intestine to go into the lesser peritoneal pouch, with fatal results. I had but one week before opened a case that had been operated on by me two years previously, the patient coming back and presenting symptoms of jejunal ulcer, in which röntgenoscopy failed to show any trouble with the small intestine. I had opened that case and found a jejunal ulcer, but found all the small intestine in the lesser peritoneal pouch. I resolved that this should never occur to me again. I made careful suture, putting in nine stitches to prevent it from occurring. Nevertheless it occurred.

Pick up the loop of the stomach in the clamp, then tack the transverse mesocolon to the stomach all the way around by a running stitch, so there is no possibility of escape of intestine into it.

I want to subscribe to the excision of ulcer whenever feasible as Dr. Knott recommends; but when the danger is greater from excision than from gastro-enterostomy, the latter operation had better be done.

Dr. Knott (closing): I wish to say to Dr. Freeman that I recommend excision only in those cases in which there is no immediate increasing danger to the patient.

As to the remarks of Dr. Harris concerning the frequency of carcinoma implanted upon the base of an ulcer: I will simply say that the literature is full of statistics bearing on that point gleaned from many clinics, and all agree that the frequency of such implantation is extreme, about 70 per cent.

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DR. HARRIS: You misunderstood me. I did not say that cancer did not have an ulcer base, but those ulcers that were cured by gastro-enterostomy did not develop cancer subsequently. That was the point.

DR. KNOTT: I am glad to be corrected. My only authority concerning that point is Moynihan. There may be others. I have his authority in his latest book on abdominal surgery. In reviewing his work in which he spoke of doing gastro-enterostomy alone, he found that in one-third of the cases recurrence took place in which pylorectomy or excision was necessary, and the remaining one-third had carcinoma, presumably developed upon the base of an ulcer, for which he made the gastro-enterostomy.

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ADDRESS IN SURGERY

Some of the Later Adaptations of Gastro-Intestinal Surgery

Charles A. L. Reed, M. D., F. A. C. S.

CINCINNATI, OHIO

Mr. President, Ladies and Gentlemen:

I beg to assure you that I entertain the heartiest possible appreciation of the invitation to address you upon this occasion. I only fear that my meager offering will be an inadequate recompense for the sacrifice of time which you make in listening to me; however, in coming before you, I feel that it is incumbent upon me rather to bring my own personal message, instead of what would probably be of more value, and that is a more or less comprehensive study, clinical, literary, and otherwise, of some specific subject.

I want to talk to you today about some things that have been more or less perplexing to me, and possibly to some of you, in my work in gastro-intestinal surgery. I feel that possibly I may be recalling reminiscences on the part of some of you when I mention a class of cases of about this type: A patient is suddenly seized with pain in the lower right quadrant of the abdomen. The pain is intense, and the patient literally shrieks for relief. His family physician is called, and finds some very constant symptoms. There is not only pain, but extreme tenderness on touch. There is

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muscular rigidity over the entire quadrant. This tenderness focalizes at the classical mid-biliary point. There has been obstipation; there is now some vomiting. After looking this complex of symptoms all over, the doctor hurriedly, and not any too hurriedly, comes to the conclusion that the patient has appendicitis. He sends for the surgeon; the surgeon arrives and looks the case over, finds that cold packs and anodynes have failed to relieve, and the patient is clamorous for relief. The surgeon at once very properly concurs in the diagnosis of the attending physician; the patient is "railroaded" to the hospital, and is very promptly subjected to an operation; put upon the table, placed in Trendelenburg's position, and classic incision is made. After fishing around for a little while a perfectly normal appendix is found, or an appendix that is possibly sufficiently catarrhal to serve as a rational explanation to the waiting friends. The patient is finally discharged and goes home and, after the lapse of a few weeks or months or a year or so, has that constitutional lack of consideration to get up another attack of appendicitis with no appendix to have it with. And then the surgeon is again called, and busies himself with explanations that are gotten up under the spur of the moment, are a trifle embarrassing, and, possibly, as little convincing to the patient and the patient's family as they are to himself.

This reminds me of a little incident that happened at the reunion of the Grand Army of the Republic, where the old boys in blue had been gathering about the camp-fire and were telling of their advances and victories and triumphs in the gloomy days of the country. They were being addressed by the late General J. D. Gordon, who was going over his celebrated address on "The Last Days of the Confederacy," and in one passage he came to a point in his narrative

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at which he had occasion to state that Sheridan's forces had retired in front of him. Just pausing there and remembering these stories that had been going on around the camp-fires all through the years, he stepped forward and said: "Look here, boys, once in a while we *did* retreat!" So now, boys, once in a while we have seen cases of this kind,—cases about as perplexing as the one to which I have alluded.

Some seven or eight years ago I had one of these cases sent me by a neurologist, who called me up and said: "This man has a fulminating appendicitis, and I wish you to come at once and take immediate steps for operation." I did so, concurred thoroughly in the diagnosis, hurriedly transported the man to the hospital, and when about ready to make incision the doctor said: "You cannot put this man in the Trendelenburg position, he is just recovering from apoplexy, and if you put his head down you will get up a degree of circulatory disturbance that will bring about a return of the trouble." "Very well," I replied, "I will operate on him sitting up." So I placed him in a semi-sitting position, made incision, pulled out the transverse colon, and in a little bit there appeared the stomach, then after a while I fished down in the bottom of the pelvis and pulled up the cecum,—very large, very flabby, very sacculated, very mobile,—and attached to it was the same old healthy appendix. And then a bright light dawned upon me, and I said to the anesthetist: "Put that man's head down if it splits wide open." He put it down, and as the order was carried into execution I saw disappearing from view the stomach and the transverse colon and the omentum, all dropping down towards the diaphragm, where they had been in all my previous operations of the kind and where I consequently never before had seen them. And then there dropped right down into the field of

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operation, right in under my incision, the mobile, flabby, sacculated, and blue cecum with its appendix attached. Of course I removed the appendix, as is the perfectly proper and gentlemanly thing to do under the circumstances. But I then and there saw that I had an explanation of the cases that had brought me embarrassment through all the preceding years. I saw that, as a result of displacement of the stomach, colon, and cecum, the condition we had been contending with was not actually a pain located within the appendix, but a pain located just at the appendix, probably by the strangulation of some sacculated portion of the cecum or possibly of a convolution of some sulcus of the ascending colon—I do not know what I had, for it all disappeared when I got to see it; but it was perfectly apparent to my mind that that was the essential underlying pathology, and it was equally apparent that my failure to relieve the essential underlying pathology in all my previous cases, explained why those previous cases had persisted in having appendicitis with no appendix to have it with.

So, the patient still with his head down, having relieved the condition below, and, making sure there were no binding adhesions, I made a transverse incision from the left costal margin just below the ensiform over the right costal margin, then obliquely down so that, altogether, I had an incision of about six inches, possibly seven. With a little flap-like arrangement, which was then made I turned this arrow; then, peeling back the epigastric fat and peeling back the peritoneum from the under surface of this and letting the peritoneum go, I brought the colon up where it belonged, and did what I have designated and what I believe I have properly designated as a retroperitoneal implantation.

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In other words, I put the colon back where it belonged; I relieved its obstructive angulation; I implanted it in a way that it would remain where it had been placed, and at the same time, by including the lesser omentum in this implantation, I secured an equally accurate fixation of the stomach. In other words, I did what in a certain sense could be designated as a gastrocolopexy, the technic of which you will find described in the *Journal of the A. M. A.* of a few years ago.

From that time on I have had no difficulty about these cases, which I believe may be correctly designated as cases of pseudo-appendicitis. I have arrived at the point where the making of a diagnosis before operation is not so difficult, because, by reason of the case cited, I was prompted to begin a study of the ptotic displacements, with the result that even in the days of dependability of the *x*-ray, which in these acute conditions is essentially impractical, in most cases with absence of distention, I am enabled to make the diagnosis. If you will just study those cases sufficiently, handle them often enough, percuss them frequently, and get used to the note of resonance that comes from the stomach, cecum, and transverse colon, you can locate these viscera with a reasonable degree of accuracy. But the one essential clinical symptom that will enable one to distinguish the difference between the pseudo-appendicitis and an actual infective appendicitis, is the absence of the initial chill and the subsequent elevation of temperature. The essential septic symptoms are absent. Then, of course, you have blood analysis and the leucocytosis to fall back upon.

Now, I do not mention this class of cases to criticize adversely the policy of operative interfer-

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ence under these circumstances. On the contrary, I believe that all of these cases should be operated upon, that they should be relieved by surgery and relieved then and there, because back of this particular strangulation you have lurking the possibility of a subsequent infection even if it has not already occurred. It is important that these cases, which are more or less evidenced by chronic invalidism, should be relieved, and this after all is the very opportune moment to do it.

But the question is, What operation shall we do? Certainly not the memorial appendectomy that it was my custom to do before doing this particular operation, because, as previously stated, this does not relieve the essential underlying pathology of these conditions. Therefore operative interference in these cases should be more extensive and should involve as nearly as possible a replacement of the organs *in situ naturale*.

There is another class of cases which we sometimes encounter. I had one but recently. The patient, residing in the South, was suddenly taken with violent stomach symptoms. She went to a neighboring city for the relief of her immediate symptom, which seemed to be very urgent. She gave the history of repeated attacks of practically the same character. She had a long history of hyperchlorhydria, she had a long history of slow gastric clearance, sometimes on one day vomiting that which she had taken the previous day. There was no history of blood in the vomitus, nor could there be obtained any history of occult blood in the stools. But she had had a number of these attacks, and they had reduced her to a state of chronic invalidism, which was accentuated by a persistent constipation. It was in this condition and with one of these attacks that she was taken down in Tennessee,

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and was removed to a hospital in Chattanooga, where she remained for a time, and was then, after subsidence of the acute symptoms, brought on a cot to Cincinnati.

I tried giving the patient a little solid food, and it immediately resulted in an attack of excruciating pain and retention, with vomiting after fifteen hours. I gave her some barium, and it was in the stomach after twenty hours. The pyloric orifice was abnormal, and there was no detection of the barium in transit through the duodenum. After a little longer treatment with gastric rest I operated upon her, having previously explained that this was obviously a case of pyloric ulcer with resulting pyloric insufficiency, and that a gastro-enterostomy should be done. My explanation was accepted, I made it perfectly lucid, drew a picture, and told them exactly what was the matter, exactly what could be done, and they could see the whole thing in advance. The patient went on the operating-table, I brought out the stomach, and there was not the slightest hint of an ulcer anywhere,—the pylorus was perfectly normal. What was the matter? I found what I might have detected by other means if I had followed up the barium indication—I found a redundancy of the colon to the extent of three and one-half feet; it was piled upon itself in the bottom of the pelvis. I found extreme traction on the peritoneal attachment of the hepatic flexure to the lower segment of the duodenum, with resulting pressure and evidences of chronic dilatation of the duodenum. In other words, here was another counterfeit presentment of a disease that did not exist. Here was a pyloric insufficiency, the result absolutely of mechanical conditions within the abdominal cavity. These were corrected. I congratulated the patient that we had all been mistaken. We were all very happy over it; she was getting perfectly well without any symptoms.

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And, fortunately, in view of what we did find, I was able to make an explanation that was satisfactory to the patient as it was to myself. And yet I confess that it is a little bit awkward to have to make these dissenting explanations in a post-operative way.

We have found some other things recently that I mention with a slight degree of hesitancy, because the work has not yet been carried to its final fruition and because, so far as its present status is concerned, it seems to be absolutely revolutionary in its tendency with respect to certain established and widely accepted views of the profession. Following the accidental discovery of gastroptosis in the case first mentioned, I began looking out for that condition, and very soon found a good many patients who did not have pseudo-appendicitis as a complication, but did have various constitutional conditions of a toxic character instead. And I began correcting these; and, after several years of this work, somewhat like a year ago next month, a patient came into my office, and she said: "Doctor, I happened to be in the city today and just came in to see you; there is nothing the matter." I did not know her when she came in, which she resented. She said: "Don't you recall me? I am So-and-so, and I came down just to tell you how well I am. I haven't had any more of those headaches; I don't have any more dyspepsia; I don't spit up my food; and I am no longer constipated, but have two or three movements of the bowels every day, and, doctor, I haven't any more epilepsy." I said, "I am very much astonished to know you ever had epilepsy." "No," was the reply, "I didn't tell you because I thought you would not operate on me." I then took her history again, and found that, previous to the operation detailed, she had been having an attack of petit mal as often as once or twice a week, rarely having an interval of

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three weeks. Eighteen months had elapsed, and she had had no recurrence. It seemed merely very fortunate; and then I recalled that in these previous years some other patients had told me that they had had epilepsy, and at the time I thought so little of it one way or the other that I had generally failed to record it. But I found the records of four such cases, and wrote to the patients. Some had had a few attacks following this operation of retroperitoneal implantation of a ptotic colon; but some of them had cleared up, some such apparent cures being of three years' standing.

It seemed to me to be a very peculiar circumstance, because I was totally at a loss to associate this condition with the more or less accepted, but, nevertheless, thoroughly chimerical pathology of the disease,—that it is presumed to be located somewhere in the head. I reported these cases, and there were brought me certain other cases of epilepsy for operation. And then I began to wonder what I should find and what the results would be. Very much to my surprise, I began finding certain definite pathological constants in these cases, all of them located within the abdominal cavity. These patients all came to me because they had, like my first case, published in our little local journal, begun with constipation, and after the constipation had existed for some time they developed the epilepsy. And then from the time epilepsy had developed up to the present they invariably found that the epilepsy was always worse when the stomach and bowels were out of fix, and the only way they could minimize the attacks was to keep the bowels open with purgatives and by such other means as they had at hand in those cases.

What have I since found? In every one of those cases I have found mechanical obstruction to the extent of inducing intestinal stasis. I have found in

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every one—exactly one hundred per cent of them—a periduodenitis with chronic dilatation of the duodenum. In the latter part of the series (in the first part I did not go far enough to look for it), I have found infiltration of the retroduodenal lymphatics, and lodged therein the bacillus lactis pyogenes, from which we make pure cultures and by the administration of which we produce convulsive attacks in guinea-pigs.

Based upon these clinical facts there is room for a very large amount of theorizing; and you are just as much at liberty as I am to formulate your own theories. But the significant fact is, that in a consecutive series of cases now numbering nearly 100, we have for the first time found pathological constants, of any character. Then we corrected the mechanical obstruction.

I said that mechanical obstruction was a pathological constant. However, the details of that mechanical obstruction may vary. You may have a markedly ptotic colon; you may find the hepatic flexure down to the level of the crest of the ilium; you will find the cecum in the bottom of the pelvis; you will find the transverse colon rolled and piled upon itself; very often you will find a lowering of the splenic flexture—I ought not to say "very often"—sometimes you will find it, not always. And sometimes you will find a very redundant colon, and sometimes you will find an exceedingly redundant sigmoid, any one of which conditions may operate in an obstructive way to retard the fecal current, produce fecal retardation with consequent hyperabsorption of the toxins contained therein. When you have a ptotic colon, especially with descent of the hepatic flexture, you find, always by virtue of its normal anatomic connections, constriction of the lower segment of the duodenum. The peritoneal ligament connecting the hepatic flexure with the posterior wall of the abdomen lies above the lower segment of

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the duodenum. From this added weight you have a certain impaction of the normal current within the duodenum. Now with this obstruction of the lower end of the duodenum and the peristaltic propulsion from above, you necessarily have a dilatation resulting in hyper-absorption under pressure. And that is why it is that in this class of cases we are finding, I take it at least, a migrated normal impairment of the alimentary canal, not previously presumed to have any pathogenic significance.

Now, then, seeing the mechanics of this condition, you can begin to understand why it is that, assuming that the microörganism in question shall be proven to have pathological significance, it may find its way into the systemic circulation and thereby become a factor in the production of one certainly of the most baffling diseases, barring cancer, with which we have had to contend. We can readily understand, furthermore, why it is that this state of affairs, resulting in the frequent explosions that are characteristic of the epileptic, sooner or later and in a perfectly secondary way, induces more or less inconstant lesions of the cerebrum. But, you say, what evidence is there that the system is thus invaded, aside from this local condition of which you speak?

In an attempt to answer this question, I will refer to another point that I have not been able to find in the books, but which I have been observing in epileptics. And let me say that I have never yet operated for epilepsy—God forbid! That belongs to the other fellow; I am dealing only with the mechanical conditions as demonstrated, and treating only the surgical conditions that are offered. But, while all this is going on, I see some other things. And sometimes these patients, while under observation, have attacks, and I find that the saliva in the midst of this does not

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give the alkaline reaction to litmus—it is always acid. What produces this acidosis? We find that urine examined immediately following these attacks carries a very large proportion of acetone.

We find another very interesting thing: That as a general rule, to which there are exceptions, and yet as a general rule, an epileptic has a mean average temperature below the normal,—97+.° Very often it will go down to 95,° then come up to 99.° When you get this extreme of temperature with extreme oppression you are in imminence of an attack, and after the attack there is but very little vacillation for several days until vacillation begins perhaps with depression, and then you have another attack. Does that look as if there was something in the way of systemic involvement in these cases?

To sum up direct observations, leaving it to you to make the analogy: You will say: Well, this is a head condition; here is a sudden suspension of consciousness.

Very well, you have a suspension of consciousness following a dose of morphia; you have a suspension of consciousness following the administration of chloral; you have a suspension of consciousness in certain systemic conditions of fairly well-established character as intoxications; and you have a suspension of consciousness in certain of the more definitely recognized infections of the septicemias in the end-histories of these cases. In other words, an infection is capable of producing a suspension of consciousness. But, you say the motor centers are involved. Yes, the motor centers are involved; the motor centers are likewise involved when you give a poisonous dose of strychnia, and you do not call that a disease of the central nervous system. You know what produced that. There is a sudden disturbance of the motor centers in tetanus,

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and you know that there is an infection involved in tetanus. In other words, the analogies are not so far removed when it comes to an explanation of the phenomena that I have recited. I will repeat: When it comes to that explanation the analogies are not so remote as they would seem at first blush.

I want to leave this question just here: The subject matter is under observation, and I bring it to the attention of this Association simply as a seed thought which I trust will germinate and result in observation. I have arrived at no conclusion, but am endeavoring, as far and as rapidly as I can, to interpret and assimilate and apply to the relief of humanity the phenomena that have come under my observation.

DISCUSSION

DR. J. W. ANDREWS (Mankato, Minn.): I do not rise to discuss this paper, but I would like to ask Dr. Reed how he recognizes this condition in the blood. If the patient has constipation and other symptoms, by what method does he definitely recognize acidosis and the degree of acidosis?

DR. ROBERT J. CHRISTIE (Quincy, Ill.): Given an epileptic case treated therapeutically with reference to acidosis, why should we not get a cessation of the epileptic seizures?

DR. JOSEPH RILUS EASTMAN (Indianapolis, Ind.): I would like to report briefly two cases in which I short-circuited after the plan of Lane with a slight modification, in that the caput coli was anastomosed to the sigmoid in two cases of epilepsy. The results, so far as the relief of the constipation which had existed in these two cases is concerned, were excellent. I never have more completely relieved the condition of stasis by an anastomosis of the caput coli to the sigmoid, than I was able to do in these two cases of epilepsy; but, as much as I would like to be able to support Dr. Reed's contentions by the citation of these two cases, I am obliged to say that, while the constipation was completely relieved, they still have epileptic attacks as violently and as frequently as before.

DR. DONALD MACRAE (Council Bluffs, Iowa): In relation to the subject of epilepsy: I would like to report two cases, one of short-circuiting of the cecum to the sigmoid, and the

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other a colectomy for epilepsy. Both of these cases came to me through having been referred indirectly by Dr. Reed of Cincinnati. These gentlemen had read about Dr. Reed's work in a local paper at home, and decided they wanted something done. One of them got along very nicely until about the third week, when, one night, he had an epileptic fit, the worst one he had ever had, and died in a short time. It is about three months since I operated on the other case, and recently, instead of having two fits a week, the patient has had an average of one seizure a month, so that I am unable to say whether this operation will be beneficial or not. This was the case on which I did a colectomy.

Dr. Jabez N. Jackson (Kansas City, Mo.): I have a theoretical suggestion to make relative to the failure in securing improvement of the acidosis from a simple short-circuiting of the bowel. In the short-circuiting operation we do not put out of commission, in a physiological sense, the ascending colon, which will still be filled by a reverse peristalsis. This is particularly the absorptive area of the gut, and, while our constipation may have been benefited, the absorptive action of the ascending colon is still maintained. Is it not possible, therefore, that the disturbing factors in these cases are essentially dependent more upon the absorption of something from the gut rather than the mere retention? If this theory be correct, it would logically follow that the proper procedure instead of any form of short-circuiting would be an extirpation of the ascending colon. The former might relieve the constipation, the latter alone will get rid of the infected area with its processes of absorption.

Dr. Reed (closing): As I stated in my paper, I did not go into the question of diagnosis and treatment of this disease. This will appear in the paper when it is published.

I look upon the saliva as the most sanitary secretion of the constitutional state, and in these cases of profound acidosis the saliva is highly and persistently acid. It should be normally alkaline. The urine is persistently acid to methyl-red as an indicator which we employ chiefly. The perspiration is very generally highly acid and under circumstances when it would normally be otherwise; but that may be a transient condition. The examination of the blood and the determination of the acidity of the blood are other methods of investigation. The therapeutic test of alkalinization is exceedingly important in determining the existence or non-existence of

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chronic acidosis, which, in these profound cases, is almost a reversible condition. To bring about alkalinization, we stop the intake of poison by keeping the colon washed out two or three times a day by high colonic lavage; then alkalinize the patients by giving bicarbonate of soda, or lithia in some form, or by putting them on the synthetic Vichy, which we find most valuable agents, and presently we will have the secretion showing up alkaline. Stop that treatment for twenty-four hours and the acidity reappears,—that is, chronic and persistent acidosis. To overcome that requires a course of treatment varying anywhere from four weeks to six months or a year. You are revolutionizing the acidity by attacking one hundred fifty or two hundred pounds of tissue, and you are trying to revolutionize or establish a constitutional condition,—a status acidosicus.

Regarding the question of epilepsy, which involves a large discussion, I shall be very brief, and will simply say that he who does simply a short-circuiting operation or a simple colectomy for the correction of the mechanical interference with the fecal current, does what? He simply stops the intake of the toxins or poisons. In these cases there are certain constant pathologic conditions present. Instead of there being uncertain pathology, there is 100 per cent pathology in epilepsy, and you will find it, in the first place, in this condition of stasis. In the next place you will find infection of the salivary glands and mucous follicles. In the third place you will find it in infection and enlargement of the retroperitoneal lymphatics. You will find impairment of the blood, and you will find present in the blood the organism of Brewer. You do not get immediate results after you have stopped the intake. Your patient has to get rid of the poison; it has to be neutralized and eliminated, and you get your results in two or three months afterwards when the constitution disposes of these conditions. In the meantime, you simply stop the intake. There is plenty of scope for other antidotal and eliminative treatment that will aid you, and he who does a surgical operation thinking he has met all indications, has stopped short of the treatment. It is not an unusual thing, I am sorry to say, that as a result of one of these operations, by virtue of the mechanical disturbance imparted to this depository of infections or poisons, you have accelerated the absorption of the poisons into the circulation and so intensified the acidosis as to deinsulate the motor tracts and thereby precipitate a convulsive seizure.

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EPIPLOITIS FOLLOWING OPERATION FOR INGUINAL HERNIA

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Diseases of the omentum are relatively uncommon, and literature on the subject is scanty, especially in the English language. Omental diseases are either tumors or inflammations. The former include carcinoma, sarcoma, lymphangioma, cysts, and teratoma. The inflammations are divided into two groups, which include, first, the cases that occur post-operatively; and, second, those not preceded by operation. Herniotomy is the operation which most frequently concerns the first group. Since ligation of the omentum is done in only a small minority of hernia operations, it follows that the complication of epiploitis is likely to be uncommon. Some men with large experience in hernia work have never seen a case.

The most recent publication on the subject of inflammatory tumor of the omentum is by L. Peterhanwahr (Archiv. f. klin. Chir., 1915, cvi, p. 355). This author has collected 44 cases, 36 of which had been preceded by operation, mostly herniotomies. Lederer, in 1908, collected 37 cases, of which 32 followed herniotomies. Lucas-Championière observed 2 cases of epiploitis among 275 operations for hernia, Dubois 1 in 300 cases, and Tuffier 1 in 600.

The French literature contains the first reference to this subject, in 1892, by Lucas-Championière, followed

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by Reynier in 1893, Forgue in 1896, and later by Boeckel, Menière, Sauget, Morestin, Le Dentu, and others.

Schnitzler's article, in 1900, was the first to appear in German. Cumston is about the only American who has written on this subject. American writers on hernia scarcely mention the possibility of epiploitis following ligation of the omentum. While it is a fact that the general use of absorbable ligature material in this country has made epiploitis a rare complication, yet it is a subject with which every surgeon should be familiar. The fact that two cases occurred within a short period in the writer's experience, justifies the belief that more cases would be diagnosed if there were a better understanding of the subject.

The case-histories which follow illustrate the acute and chronic types of the disease.

CASE 1.—A young man had been operated upon for a moderately large left inguinal hernia. A large piece of omentum was removed en masse with a heavy iodin catgut ligature. The patient progressed normally for eight days, at which time the wound had healed by first intention. The patient then became feverish, and complained of abdominal pain on the left side. Nausea developed and the abdomen became distended and the bowels inactive. The symptoms increased in severity, and the pain became generalized.

When seen by the writer on the twelfth day the condition of the patient was desperate. He presented the typical picture of a general peritonitis. To what extent, if any, the peritonitis owed its existence to the preceding hernia operation it was impossible to determine.

The abdomen was immediately opened through a left rectus incision at the level of the umbilicus. The peritoneal cavity was full of pus under high tension, and there were fibrinous flakes in the fluid and on the surface of the organs. The omental stump was found in the upper left quadrant of the abdomen. It was a mass the size of a lemon, and was covered with exudate and saturated with pus. It was not adherent to the abdominal wall, but was closely

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applied to the transverse colon. The catgut ligature was still demonstrable. A hurried search was made for bowel perforations, but none were found. There was nothing to indicate any injury to the bladder. The abdomen was thoroughly drained, but the patient died in twenty-four hours.

This virulent peritoneal infection could have been introduced in no other way than by the retracting omental stump, and it is remarkable, yet typical, that the field of operation escaped contamination.

Case 2.—A man, about forty years of age, was operated on for a large right inguinal hernia, and a portion of the omentum was removed, catgut ligatures being used. The wound healed by primary intention, and the patient left the hospital in two weeks. He was seen by the writer about one month later, and was complaining of not having felt well for two weeks. There was a feeling of general malaise, loss of appetite, constipation, and pains on the right side of the abdomen.

On examination, the hernia wound was found to have healed perfectly. There was a low grade of fever, 99°-100° F. The urine was negative, and there was a leucocyte count of 10,000. Examination of the head, chest, and extremities gave negative results. The abdomen was slightly distended, and the muscle reflex on the right side was exaggerated. To the right of the umbilicus could be felt a mass about four inches in diameter, and was fairly well circumscribed. It was very tender on pressure, and did not move with respiration. There was dullness on percussion, and this dull area was separated by a zone of tympany from the liver. There was nothing about the findings to associate the tumor with the liver or kidney. It was located rather high for the appendix. To one familiar with epiploitis in this connection, the diagnosis would have suggested itself.

The abdomen was opened through a right rectus incision directly over the tumor, which was found adherent to the abdominal wall. The adhesions were not firmly organized, and were easily separated both from the parieties and the underlying bowels. Upon delivery, the tumor was found to consist of a mass of inflamed omentum. Almost the entire omentum was involved. There was just enough healthy tissue intervening between the inflammatory tumor and the transverse colon to permit ligation. Examination of the inguinal region from the peritoneal side revealed a normal

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condition, and no sign of a local abscess. Nothing further being found, the wound was closed without drainage, and the patient made an uneventful recovery, and was relieved of his symptoms.

The removed tissue was hard and dense, being largely fibrous with fatty areas interspersed. In the central portion were several areas of softening, but no sign of ligature material was found. The pus from the softened areas gave a growth of staphylococcus aureus.

This case is a typical example of the most commonly observed type of epiploitis,—the subacute variety. It is worthy of note that catgut, and not silk, was the material used for ligation. The presence of infection in the center of this mass would seem to justify the assumption that spontaneous recovery would not have occurred. The process would have dragged on until the formation of an abscess made interference imperative.

A number of types or varieties of epiploitis are described by the various authors, but Peterhanwahr offers the best classification, which is as follows:

1. Post-operative variety.
   (a) Simple inflammatory hyperplastic type.
   (b) Suppurative epiploitis.
2. Extension from neighboring organs, not following operation.

Sauget makes three subdivisions which are very similar to the above:

1. Epiploitis plastica simplex.
2. Epiploitis plastica with adhesions.
3. Epiploitis purulenta.

The variety most commonly observed is the simple inflammatory hyperplastic. The free interval after operation before the onset of symptoms is variously reported at from five days to three years. In the majority of cases it will average from three to four weeks.

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The type of inflammation which develops in the given case depends upon the virulence of the infection or the degree of mechanical irritation. In the very acute cases, of which an example is reported in this paper, the acutely inflamed omentum is but part of a general peritonitis. As the process becomes more chronic, the inflammatory products become more organized and fibrous. The mass may look like a fibrolipoma; and on section one or more abscesses are usually found with the ligature as a nucleus. Adhesions to the abdominal wall are almost invariable, and at times large abscesses have developed which were opened by the surgeon or found a spontaneous exit. Sinuses have sometimes persisted because of the presence of masses of silk.

Finally, there is the slow-growing chronic form, which so much resembles a malignant tumor that diagnostic errors have occurred resulting in unnecessary resections of the bowel. Its structure is so dense and fibrous that it appears like a sarcoma or a carcinoma, and this resemblance is increased by the total absence of pus. It is believed by many that the mechanical irritation of the silk alone, without any accompanying infection, is capable of developing the large fibrous growths which have been observed.

The following are the main factors concerned:

1. The ligature material.
2. The manner of ligation.
3. The condition of the omentum at the point of ligation.
4. Infection.

All of these factors are usually involved, although one or the other of them may, in the individual case, be playing a more prominent part.

1. The ligature material used in about 90 per cent of the reported cases was heavy silk. (Reynier,

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Monod, Guinard, Braun.) In a small percentage, kangaroo tendon, chromic catgut, or iodin catgut was employed. Thus it is evident that the trouble has its origin mainly in the non-absorbable material, plus infection. Holländer, however, believes that sterile silk may act as a mechanical irritant, as a result of which a large mass may finally develop. His theory is, that, while the ligature may be drawn tightly enough for hemostasis, it is still loose, and necrosis of the stump does not follow. The tissues are then stirred into activity by the constant irritation of the unabsorbed ligature, and a fibrous tumor-like mass is the result.

Sterile absorbable ligatures of small size will cause no trouble, but the addition of infection will result in inflammation irrespective of the nature of the ligature material.

2. The manner of ligation has had much to do with the result. The case-reports are almost unanimous regarding the large amounts of omental tissue included in the ligature, placed either en masse or as a figure eight. In some of the cases reported the entire omentum had been included in one ligature.

3. The condition of the omentum at the point of ligation is a matter of great importance. Thus, if section is made through diseased tissue, such as is often found in old incarcerated hernias, the retraction of the pedicle will be followed by a spread of the inflammation to the rest of the omentum. Reynier, Roche, Boeckel, and others report cases of this kind following the use of silk.

4. There can be no doubt that infection is the most important factor of all. In some cases it may be of low grade, and gradually lose its virulence, lending support to the belief that the connective tissue was the result of mechanical irritation alone. It is no unusual thing for non-absorbable foreign materials of various

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kinds to heal in the tissues, and remain passive indefinitely, provided they were sterile. The addition of infection makes it impossible for any foreign material to remain quiescent.

That the field of operation in the vast majority of reported cases remained clean seems extraordinary. This would tend to show that there was something inherently susceptible about the omentum, rendering it more subject to mechanical, chemical, or bacterial insult than the tissues of the groin.

Symptoms.—In the very acute cases, after an interval of several days following operation, there will be the usual signs of an acute infection within the abdomen, ending in general suppurative peritonitis. In rare instances the process becomes localized with abscess-formation.

By far the majority of the cases run a subacute or chronic course, lasting for months, even years. The free interval after operation is generally from four to eight weeks, but cases are on record where two to three years elapsed before the onset of symptoms.

Pain is one of the first and most constant symptoms. It is on one side or the other, depending on the location of the trouble. In most cases it is in the upper half of the abdomen, and is aggravated by deep breathing, coughing, or tight clothing. Associated with the pain there is usually a rise of temperature, with all the symptoms that go with fever. There are no definite signs pointing to any special organ; and, after continuing for a variable time, the symptoms may gradually subside, and the patient get well. At other times the disease is progressive, and all the signs of a localized abscess in the abdomen develop. In such cases where operation had been refused, the pus found an outlet through the bowel, or burrowed through the abdominal wall.

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In the very chronic cases the onset is very gradual, and the symptoms are vague and indefinite. The pain and gradual loss of weight are very suggestive of cancer, especially if there is a palpable tumor.

Diagnosis.—In every case of herniotomy where abdominal symptoms supervene, the possibility of epiploitis must be considered. Undoubtedly, cases have not been recognized, because the subject has received but little attention. Associated with pain is the development of a mass in the abdomen, which is frequently first discovered by the patient himself. The tumor is usually on the same side as the herniotomy, presenting in the hypochondriac or umbilical region. Very rarely has the tumor been found in the inguinal region or in the pelvis. The mass may be small, but it has been known to assume huge proportions. On palpation, there is tenderness, and the tumor is usually but slightly movable, nor does it move with respiration. Percussion will elicit dullness, because the bowels lie below the mass.

In the differential diagnosis, such diseases as cholecystitis, subacute appendicitis, chronic perforation of the stomach or duodenum, foreign body (sponge), diverticulitis of the sigmoid, and others must be considered and eliminated.

Treatment.—According to Braun the development of an inflammatory tumor of the omentum after herniotomy does not necessarily demand operation, for a considerable proportion of the cases have been known to recover under expectant treatment. Naturally in some of these recovered cases, there exists some doubt as to the correctness of the diagnosis.

The most radical view was expressed by Renier, one of the early writers, who advised extirpation of the tumor as though it were malignant.

The practical lesson which we are taught by this

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study concerns the matter of prevention. Epiploitis, as a post-operative complication of herniotomy, can be avoided by the observance of a few very elementary rules. First of all, the aseptic and operative technic must be as perfect as possible. Fine catgut should be given the preference over the heavy sizes. Only small bits of the omental tissue should be tied off at a time, never employing the notorious en masse or figure-of-eight ligature. Finally, section should always be made well within the limits of healthy tissue.

The decision for operative interference in cases of epiploitis rests with the judgment of the surgeon, based upon the severity of the symptoms and the progress of the disease. In case of doubt, no harm can come from an exploration. If it is found that the inflammatory mass can safely be removed in toto, this would best be done. In the presence of extensive adhesions and abscess-formation, interference should be limited to drainage. In the event of intestinal obstruction resulting from pressure by old fibrous tumors or from cicatricial contraction, it might become necessary to establish an anastomosis or do a resection of the bowel.

BIBLIOGRAPHY

Reynier: Du danger de la ligature de la soie dans la résection de l'epiploon enflammé 9th French Surg. Cong., 1893.
Forgue: Constitution a l'étude des épiploites tardines consecutives a la kelotomie et a la cure radicale. Thèse de Montpellier, 1896.
Boeckel: Revue de Gynécologie, 1897.
Mencière: Epiploites tardines consecutives a la cure radicale ou a la kelotomie. Gaz. hebd. de méd. et chir., 1897.
Schnitzler: Ueber Epiploitis im Anschluss an Operationen. Wien. klin. Rundschau, 1900.
.....: Epiploitis nach Herniotomie. Wien. klin. Wchnschr., 1907.
.....: Arch. f. klin. Chir. Bd. 59.
Haberern: Ueber entzündliche Geschwülste des Netzes. Deut. med. Wchnschr., 1908, No. 48.
Peterhanwahr: Ueber entzündliche Geschwülste des Netzes. Arch. f. klin. Chir., 1915, cvi, 355.
Leroy: Les Epiploites. Arch. gen. de chir., Paris, 1907-8, I.
Delore: Des epiploites post-operatoires. Lyon méd., 1911, cxvi.
Valence: Epiploite suppurée consecutive a une cure operatoire de hernie inguinale ouverture dans la vessie; guérison. Archiv de méd. Nav., Paris, 1908, xc, 98.

Digitized by Google

Proust: Bull. et mém. de la Soc. de chir. de Paris, 1908; vol. 33.
Léjars: Bull. et mém. de la Soc. de chir. de Paris, 1908; vol. 34.
Hadra: On omental tumors due to adhesive inflammation. Annals Surg., 1891.
Reclus: Semaine méd., 1899, p. 63.
Wieting and Risa: Zentralbl. f. Chir., 1904, p. 1309.
Lederer: Ueber Epiploitis. Centralbl. f. d. Grenzgeb. d. Med. u. Chir., 1908, xi.
Zesas: Ueber im Anschluss an Bauchoperationen und Entzündungen der Bauchorgane vorkommende entzündliche Geschwülste des Netzes. Deutsch. Ztschr. f. Chir., 1909, xcviii, p. 503.
Cumston: Epiploitis following the radical cure of hernia. International Clinics, 1909, iv, p. 145.
Holländer: Zur Genese der Netztumoren (Epiploitis plastica). Deutsch. med. Wchnschr., 1913, xxxix, 845.
.....: Zur Frage der Geschwulstbildung nach Netzunterbindungen. Berl. klin. Wchnschr., 1913, i, 511.
Becker: Chronisch entzündliche Netzgeschwülste. Berl. klin. Wchnschr., 1913, i, p. 1903.
Braun: Ueber entzündliche Geschwülste des Netzes. Archiv. f. klin. Chir., 1901; Bd. 63.
Renzi and Boeri: Berl. klin. Wchnschr., 1903, No. 39.
Simmonds: Münchn. med. Wchnschr., 1907, p. 635.
Tuffier: Bull. et mém. de la Soc. de chir. de Paris, 1899.
Guinard: Epiploites, consecutives aux ligatures a la soie.
Leroy: Arch. gen. de chir., Aug. 1907, p. 78.
Semaine méd., 1899.
Hamann: Surgical conditions of the great omentum, Ohio State Med. Jour., Nov., 1915, p. 685.

FOR DISCUSSION SEE PAGE 307

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THE EARLY DIAGNOSIS OF INTUSSUSCEPTION IN CHILDREN UNDER THREE YEARS OF AGE

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The following observations were made upon twelve infants, none over three years of age, and all, except three, operated upon during the last two years. Six were operated upon during the last year. Four cases were under the care of competent surgeons who kindly furnished records; one case was operated upon by my colleague, Dr. S. H. Baxter. Seven cases came under the care of the writer.

Forty years ago, the eminent Charles West, of London, had seen only five cases of intussusception among forty thousand sick children examined by him.

In a children's hospital in a large Eastern city only twenty-seven cases were recognized in seventeen recent years.

In the cases cited in this report the diagnosis was made or confirmed in all but two by Dr. J. P. Sedgwick, Professor of Pediatrics in the University of Minnesota, or his colleague, Dr. F. C. Rodda.

This shows a decided improvement in diagnosis, especially by skilled pediatrists, but it cannot be doubted that too many babies have died, supposedly of dysentery or enterocolitis, who might have been saved by an early diagnosis and good surgery.

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The age limit of three years in this report was selected, because, in infants of three years or under, the diagnosis is vastly more difficult, for we cannot depend upon their intelligence, but must make the diagnosis entirely upon their condition and clinical behavior. Operations upon children over three years old, and therefore intelligent enough in many instances to aid in the diagnosis, are for this reason not included in this report.

Two were four months old.

Two were five months old.

Three were six months old.

One was seven months old.

One was ten months old.

One was sixteen months old.

Two were thirty-six months old.

Nine, or 75 per cent, were under one year.

Three cases had had a diarrhea a short time before.

The intussusception was in all twelve cases ileocecal.

All of the cases, 100 per cent, began suddenly with violent pain, apparently abdominal.

In three cases, 25 per cent, this pain was immediately followed by collapse, which continued up to the time of operation.

In eleven cases, 100 per cent of those in which this sign was noted, the initial pain was followed by regularly recurring pains. In one case we have no record.

Two at no time had blood in the stool.

One first had blood three hours after attack.

One first had blood ten hours after attack.

One first had blood two days after attack.

Three first had blood three days after attack.

One first had blood five days after attack.

Seven out of nine, or 77 per cent, either had no blood in their stools or not until after two days.

Ten, or 83 per cent, had mucous stools more or

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less mixed with blood as just indicated. The time when the mucus appeared in the stool could not be ascertained. In most cases it was present when the patient was first seen by the physician and before the blood appeared, and hence may be regarded as an earlier symptom than the bloody stools.

In only one case was there any fecal movement after the beginning of the attack.

Not taking into account the emptying of the stomach of food at the onset, which is almost without exception:

One vomited continuously from the first.

One did not vomit at any time.

One vomited occasionally after 12 hours after attack.

Three vomited occasionally after 24 hours after attack.

Two vomited occasionally after 36 hours after attack.

Two vomited occasionally after 4 days after attack.

One vomited occasionally after 5 days after attack.

One, no record.

This secondary vomiting was absent or did not come until after 24 hours, in nine out of eleven, or 81 per cent. In most of the cases it occurred only after the ingestion of food or drink.

Six cases had no fever. Four cases had slight fever, 100° F. and 102° F., beginning during the third day or after.

The cases with fever were those which were first observed several days after the attack, or had had, just preceding, an enterocolitis or bronchopneumonia.

In eight cases out of ten, 80 per cent, the abdomen was flaccid.

In two cases, slightly rigid.

In two cases, no report.

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The examination for rigidity was made between the paroxysms of pain.

Abdominal tumor was noted in eleven cases, or 92 per cent. It could not be felt in one case.

In eight cases the tumor was in the left lower abdomen.

In two cases, in the ileocecal region.

In one case, in the course of the transverse colon.

It must be admitted that the presence of a tumor was rather doubtful in three of the cases, yet decided upon affirmatively before operation.

The intussusceptum in the eleven cases examined was felt, per rectum, in six, 55 per cent.

Not found in five cases, 45 per cent.

No examination in one case.

Gas explosions, loudest near the ileocecal region, were heard in all cases examined. Unfortunately no record was kept of these examinations.

In children so young it is usually impossible to say whether or not one finds a localized tenderness. Occasionally one may be certain that it does not exist. In four of our cases this absence of tenderness could be definitely established. In all of the cases there was apparently no more tenderness over the tumor than in other parts of the abdomen.

Any one who is familiar with the examination of little children perfectly understands how little one can depend on this sign.

In only two cases was there evidence of peritonitis at time of operation.

Studying these symptoms more in detail, we find that these little children, apparently in perfect health, were suddenly stricken with a violent abdominal pain and a regurgitation of the stomach contents. At this point we must recognize that the symptomatic path

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divides, and we have to consider two courses the disease may take.

In one, the little one recovers its cheerfulness in a few moments, and very likely begins to act as usual and perhaps, if old enough, resumes its play. In a little while it again cries out, and this alternate relief and pains recurs again and again.

In the second form the child with its first pain goes into a collapse, its pale, pinched face and rapid pulse indicating profound shock.

The first form, these recurring pains, with periods of relief, the writer regards as the most usual and conspicuous of symptoms. The outcries coupled with the pains are peculiar, and vary from a little grunt to a subdued moan. I have never heard one of these little ones scream. Especially noticeable are their accompanying grotesque gestures and positions, such as limping about with the hands on the abdomen, or boring into the pillow with the head, the child taking the knee-chest position. The child may roll, face down, on the floor or throw itself across its mother's knee. The sudden relief between the pains is equally striking.

These pains last from one-half to three minutes, rarely more than two, with a longer interval of relief.

Dr. Sedgwick made a diagnosis, in one of the cited cases, from the character of the pains alone. These pains are as regular in periodicity as labor pains, unlike those of common bellyache or colitis. They are not continuous with exacerbations as in appendicitis. They naturally resemble the pains of other mechanical intestinal obstructions, but all other mechanical obstructions are exceedingly rare in children under five years of age, hernia excepted.

In the second form, that of collapse, the child is often too weak to manifest signs of pain beyond occasional moaning. This symptom, sudden collapse,

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with periodic cries, should, in a child previously normal, strongly suggest the probability of an invagination of the intestine.

Next, in frequency but not in importance, are mucous stools, 100 per cent in the ten cases in which we have a record. This mucus is often streaked with blood and would be of the utmost diagnostic importance were it not for the fact that it is almost equally as often seen in dysentery. It is not one of the earlier symptoms, but earlier than the bloody stools. It is rarely noticed before the second day, and often not until the third.

The next most common symptom noticed in our cases was the presence of tumor, 92 per cent. The presence of a tumor should not be negatived except after the most careful and gentle palpation, made during the interval between the pains. The time when a tumor is first noticeable can of course be seldom determined, as the child is not examined for this sign until seen by the physician, and then often not until the symptoms are urgent. In one case, the child of a physician, the tumor was made out in three hours after the initial pain. Before the swelling of the intussusceptum takes place there can be no discoverable tumor. When the swelling takes place there must, of course, be a tumor, but it may be so soft and so situated that it cannot be demonstrated. Considerable swelling of the distal end of the intussusceptum undoubtedly occurs within three hours in most cases. If we could always identify this mass, we should have, in it, a very early and important symptom.

The next most common symptom, 83 per cent in our cases, is the absence of fecal movements. The babe may have one or two movements immediately following the attack, but after that the bowel is empty below the intussusceptum.

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A flaccid abdomen, generally scaphoid, was noted in 80 per cent of our cases. It is noted early and may continue for days. Distention comes too late to be of diagnostic importance.

Rejection of the stomach contents accompanied the onset in 100 per cent of these cases, but vomiting was not resumed until after the second day in 81 per cent. In one case there was no vomiting. Two did not vomit until the fourth day, and one not until the fifth. One vomited continuously from the beginning. We cannot, therefore, regard vomiting as an early or constant or reliable symptom.

Blood in the stools was absent for the first two days in 72 per cent. When present, this is, of course, a most convincing symptom, but, contrary to the general impression, is not an early one and its absence should not invalidate the diagnosis. Two cases had no blood in the stool at any time, in three the blood did not appear until three days, and in one not until the fifth.

When the intussusceptum can be positively identified by rectal touch, it is absolutely pathognomonic, but it could be reached by the finger in only 55 per cent of our cases. The absence of this sign should by no means exclude intestinal invagination as our statistics show that we may not expect to find it in almost one-half of cases proved, by operation, to be intussusception.

Loud gas explosions, as heard in the ileocecal region by the aid of the stethoscope, we believe is an early and significant symptom that has not received the attention that its importance would suggest. Unfortunately, no record was kept of the number of cases which were examined for this sign. These loud borborygmi are, of course, like those heard in other mechanical obstructions of the bowel, but such obstructions are practically absent in these infants, with the

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possible exception of hernia, which any surgeon would naturally have in mind in making his diagnosis. These sounds are loud and not at all like the whistling, squeaky sounds heard in flatulency or in inflammatory conditions.

Osler, writing of intussusception, without regard to age, says that "peritonitis usually develops in two or three days." In our twelve cases there was no evidence of peritonitis, notwithstanding the intense mesenteric strangulation in some, with two exceptions. In these cases the babes had been sick at least five days. It may be that peritonitis due to intussusception develops earlier in the adult than in the infant.

Reviewing those symptoms as to the period of appearance and importance, we find:

1. That a sudden violent abdominal pain, accompanied by a regurgitation of its stomach contents, in a child otherwise well, initiated the attack in 100 per cent of the cases.

2. That recurring pains, varying in intensity but regular in periodicity, accompanied by the assumption of peculiar positions, generally prone in those strong enough to move about, occurred in 100 per cent of those noted. That in those cases beginning in collapse, 25 per cent, these periodic pains were very often indicated only by regularly repeated moans and drawing up of the limbs.

3. That an abdominal tumor could be made out somewhere in the course of the colon in 92 per cent.

4. That the stools did not contain feces in 91 per cent.

5. That mucous stools are recorded in 83 per cent.

6. That the above indications should strongly suggest intussusception within 48 hours after the attack.

7. That blood in the stools adds to this certainty

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of diagnosis, but it may be absent in 77 per cent until after the second day.

8. That instead of distention we may expect a flaccid, scaphoid abdomen.

9. That recurring vomiting is not usually one of the earliest symptoms, being absent in 81 per cent until the second day or later in our cases, and that in exceptional cases there is no vomiting.

10. That the positive identification of the intussusceptum by the finger in the rectum is absolutely pathognomonic, but may be demonstrable in only 55 per cent.

11. That the virulence of the disease and its mortality depend not so much upon the time elapsed before operation as upon the intensity of the strangulation of the mesenteric circulation.

12. That, to protect all cases, the earliest possible diagnosis and operation is imperative.

This observation is supported by the following mortality account:

Of the 12 cases, 8 recovered, 4 died.

Of the non-collapsing cases, 8 recovered, 1 died.

Of those beginning in collapse, none recovered, 3 died.

DISCUSSION OF THE TWO PRECEDING PAPERS

Dr. M. L. Harris (Chicago, Ill.): An interesting point in connection with epiploitis, as brought out by Dr. Hessert, is the fact that in practically all of the cases the wound of operation remains sterile while the omentum becomes infected. This should lead us to search for the manner in which the omentum becomes infected while the wound remains sterile.

One possible method of infecting the omentum is drawing it out and laying it on the abdomen, as undoubtedly we have all seen at times, while ligating it off. In that way we come in contact with the skin; and the omentum may become infected from the skin. The fact that the wound remains sterile while the patient develops some intra-abdominal trouble has in these cases led us away from a correct diagnosis. Examining the wound and finding it perfectly clean, some other

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cause is sought to explain the pathology which has taken place.

The point which the doctor has made with reference to handling the omentum and how it should be ligated, is well taken.

The other point I wish to make is, that the omentum should not be removed unless it is absolutely necessary. If the omentum is adherent to the sac, it must be detached, and that will require the application of a ligature. If there is the development of hyperplasia, and frequently the accumulation of a large mass of fat, and the parts are constricted, that mass of fat must be removed; otherwise the mere fact that the omentum is in the hernial sac is no indication for its removal, and it should not be removed if it can be replaced without difficulty.

Dr. Daniel N. Eisendrath (Chicago, Ill.): I want to place on record a case similar to the acute variety that Dr. Hessert spoke of. It was a case that occurred in the clinic of Dr. Steele about ten years ago while I was his chief of clinic. It followed a right herniotomy, and gave rise to all the symptoms spoken of, namely, those of an acute, right abdominal lower-quadrant inflammation, with high temperature coming on about a week following operation. We made a right rectus incision in the omentum and found a collection of pus such as the essayist mentioned.

In regard to the paper of Dr. Abbott: He is to be congratulated on having a mortality of only 25 per cent in twelve personal cases of intussusception below the age of three. That is better than the average in a large city.

I wish to place on record the case of a three-months' baby operated on the third day of an intussusception. I was confronted with the choice between resection and an artificial anus. I resected about twelve inches of the ileocolic region, made a lateral anastomosis, and the child recovered.

There is one thing I want to mention, and that is, those babies brought in with intussusception and those who have congenital pyloric stenosis, have not the reparative power that the normal baby has. I have sutured up their wounds in layers, but in spite of this the edges will separate and evisceration take place. The best way to do, when these eviscerations take place, is to make a pressure with fingers, and not try to suture. Simply replace the prolapsed intestines, hold them in, and bring the wound-edges together with adhesive plaster. I have made it a rule in all these

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toxic children to forestall this trouble by putting on strips of adhesive plaster to re-enforce the abodminal incision, and have not had any further trouble.

In regard to the diagnosis: I am very glad indeed that Dr. Abbott disturbed the fallacy of thinking we must have blood in the stools and an abdominal tumor in these cases. The trouble is that the diagnosis is not made early enough yet to reduce the mortality much. It is not right to wait forty-eight hours to make the diagnosis. The diagnosis, to be of value, in order to reduce the mortality, should be made in the first twelve hours.

Dr. Arnold Schwyzer (St. Paul, Minn.): The value of Dr. Abbott's paper, it seems to me, lies in the fact that he has analyzed the different symptoms and has told us what the percentage of them is, so that we know which we have to look for first, which one is constant, and which may be absent. He mentioned initial pain. That really is the first symptom. It is the most important symptom, and ought to lead us in the right direction, because these patients are usually well-fed children. They are usually breast-fed children, and have been well up to that moment, and there is no ordinary plausible cause for the pain from which they suffer. This pain is sudden, sometimes persistent. If the child has been kept on artificial foods the significance of the vomiting, which comes on with the initial pain, does not amount to much, nor does the pain signify much.

In connection with palpation Dr. Abbott mentioned the introduction of the finger into the rectum. That is the most important examination. If the invagination has reached the rectum, the case is, as a rule, quite advanced. Gentleness and taking enough time are important factors for success of the abdominal palpation. The tumor is usually not at the ileocecal area, but farther up and under the ribs. With the finger in the rectum, but examining bimanually, we are liable to get information which we would not get otherwise.

Another diagnostic help which ought to be mentioned is the *x*-ray. In a doubtful case it might be of importance. With an enema of bismuth or barium we can get complete and exact pictures of the colon, and derive very definite information of what we have.

Dr. William Fuller (Chicago, Ill.): I rise to say a word regarding the paper read by Dr. Hessert. I think the condition he describes bears a close relation to another rare condition in the abdomen, and that is torsion of the omentum. I

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have never seen a case such as Dr. Hessert has described, except as a part of the clinical picture of a general diffuse suppurative peritonitis.

I have seen one case of inflammation of the omentum which resembled in part the condition described by Dr. Hessert, but that case was a typical torsion of the omentum,—a torsion of the omentum not related to a hernia, the patient, in other words, had no hernia. This case of intra-abdominal omental torsion was reported some four years ago. A few years previous to this report, Dudgeon and Sargent reported some sixty or seventy cases of omental torsion, all occurring as a result of, or in close relation to, hernia.

There are two very significant things in these reports of Dr. Hessert. One is that both cases followed operations for hernia; the other is that an infection followed closely upon the operation, and involved the omentum, but not the hernial structures. If we bear in mind the capacity which the omentum possesses for taking care of an infection without giving rise to pronounced clinical manifestations, and therefore a greater resistance to infection than the hernial structures, we would look for trouble in the latter tissues from whatever source the infection before we would in the omentum.

In listening to the paper, and from a privilege I had of previously reading it, and that both cases here reported followed hernias, the condition so frequently associated with omental torsion, as well as the very great rarity of epiploitis, the thought suggests itself that these cases might have been in the beginning at least partial or complete torsions of the omentum.

Perhaps the distinction after all is not a great one, as the conditions are similar in symptomotology and call for practically the same treatment. The doctor's advice in operating on cases in which the omentum must be dealt with is worthy of consideration and should not be forgotten.

Dr. Miles F. Porter (Fort Wayne, Ind.): I would like to call attention to the advisability of making a digital rectal examination with the patient in the upright, instead of the horizontal, position.

Dr. Abbott (closing): I wish to thank Dr. Schwyzer for mentioning the *x*-ray. It would probably be of advantage in some cases. In all of our cases we got a positive diagnosis without it.

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INTESTINAL STASIS

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Since Sir Arbuthnot Lane has made intestinal stasis fashionable the literature has been so voluminous that it requires courage to appear with a paper on this subject. It is my purpose to attack the subject from a somewhat different angle than the usual one, and to present the arguments along new lines.

The usual thing is meant by intestinal stasis, and, although I can hardly accept all the various effects Mr. Lane enumerates, it is easy to concede that an aggravated condition of intestinal stasis is capable of producing most serious symptoms. No enumeration of these symptoms is necessary here, but let it be understood that the discussion is intended to include all the cases, trivial and severe, of this exceedingly common combination of symptoms.

In the writer's conception of intestinal stasis the condition falls naturally into three classes:

1. Cases due solely to mechanical causes.
2. Cases due solely to weakened and dilated intestines without mechanical obstruction.
3. Cases primarily of the second class, but with adhesions resulting from subinfections.

There is very little to be said regarding the first class. If inflammatory adhesions, persisting bands, membranes, or kinks produce enough mechanical interference to the progress of the intestinal current to

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result finally in a weakening of the intestinal walls and a gradual lessening of peristaltic efficiency, the result will be intestinal stasis with all the varied train of symptoms that we have learned to expect. In such cases the etiology is plain, and the treatment is not a subject for discussion. The mechanical cause of the stasis must be removed surgically; and if this is done effectually and reasonably early the results are perfect restoration to health and normal function.

The second class is the one to which I wish to direct your attention. Normally, the food is taken into the stomach with appetite, the saliva and gastric juice are secreted in sufficient abundance, peristalsis is started, the bile and pancreatic juice appear at the correct stage of the procedure, the intestinal juices bear their share, and sufficient gas is formed to aid materially in giving safe passage of the food along its way. Even the normal tonicity of the abdominal muscles is an important factor in helping the intestinal contents along the journey to the large bowel. When the colon is reached, the food is ready for absorption. Finally, the residue is deposited in the descending colon, which is to be considered a mere reservoir. Defecation is the final act which leaves the colon ready to receive the next installment. As long as all the functions are perfect the process of digestion, absorption, and discharge of the waste material goes on in a natural way, and none of the organs concerned in the process is being overworked, weakened, or exhausted. A perfectly sane and well-ordered life and dietary ought to go on a lifetime without a hitch.

Unfortunately, our boasted civilization makes it next to impossible for anyone to permit his engine to run along normally. Our modern dietary would make a Hottentot cry for help before he had been living a week in an up-to-date hotel or in a well-ordered civil-

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ized home. The food is too highly seasoned, too great in quantity, too heavy, too difficult to digest, and probably not masticated sufficiently. The result is, it reaches the colon not fully prepared for absorption. It probably remains in the descending colon and sigmoid too long and in too great quantity; and an excessive amount of gas forms, the colonic wall becomes overdistended, and the musculature becomes overtired.

Many persons eat too much and too improper food, and fail to take enough physical exercise. The result is a gradual weakening of not only the abdominal muscles, but the muscles of the intestinal walls, thus enfeebling peristalsis. When this condition of affairs is continued long enough a pathological circle is started which tends to be self-perpetuating. The nutrition absorbed becomes less, the musculature of the body becomes steadily weaker, the power of digestion becomes poorer, evacuation of the waste becomes more and more delayed, toxic material in the colon increases in quantity, and the tissues are being more poisoned as long as this vicious circle is permitted to exist.

The above description applies to the simplest form of the trouble, and the one most easily prevented. There are some conditions affecting digestion that are potent factors in determining the normal digestion, assimilation, and expulsion of waste; and the absence of these conditions is just as potent in producing intestinal stasis and all its evil consequences.

Unfortunately, until we know more about the exact action of the intestinal nerves,—how they control the secretory, absorptive, and motor functions of the intestinal canal,—it will be impossible to gain a complete knowledge of the steps from perfect health to an advanced state of intestinal stasis. Not much is definitely known, but a rough sketch of the stages in the development of the condition may be drawn.

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Pawlow, experimenting with dogs, found if they were given food they enjoyed it, and it excited, not only the flow of saliva, but of gastric juice as well. It was so arranged that the food did not pass into the stomach, but, nevertheless, the secretion of gastric juice occurred as the result of the mastication of appetizing food.

Bidder and Schmidt went a step farther, and showed that the mere sight or smell of appetizing food was capable of exciting the secretion of gastric juice. It was also shown that the presence of gastric juice on the mucous membrane of the duodenum was necessary to excite the flow of the bile and pancreatic juice. It is also well established that the secretion of the intestinal juices, and even gastric and intestinal peristalsis, are largely influenced by the normal secretion of the gastric juice, bile, and pancreatic juice. The normal carrying on of all these vegetative functions concerned in digestion and assimilation are so closely interrelated and dependent on one another that one break in the chain seems enough to stop the whole process.

Cannon has shown that the state of the mind and the emotions is of the utmost importance to the digestive and assimilative functions. Even when the food is pleasing, if the diner is affected with unpleasant emotions, as vexation, worry, or anxiety, or with great emotions, as anger or fear, the mouth becomes dry, the gastric juice is scanty, and the whole process of digestion and assimilation is stopped or rendered very tardy and imperfect.

LeComte showed that, when a dog was eating and the gastric juice was secreting normally, by the introduction of the element of great fear the secretion of the gastric juice ceased at once. Pursuing his investigations, a dog was thrown into violent anger by a cat. The dog was then fed, and, being very hungry, ate

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ravenously. No gastric juice was secreted worthy of mention. During a perod of twenty minutes only a small amount was secreted, and that was mostly mucus.

Oeschler has reported that such psychic disturbances as fear or rage will also stop the secretion of the pancreatic juice and of the bile. It has thus been adequately proven that all the factors which produce the chemical changes in the food in normal digestion may be abolished by psychic disturbances.

But Cannon has also shown that "just as there is a 'psychic secretion' so likewise there is a 'psychic tone' or 'psychic contraction' of the gastro-intestinal muscles as a result of taking food." He demonstrated that in the cat any sign of rage or fear was accompanied by complete absence of the churning waves. He showed the same in the rabbit, dog, and guinea-pig. Continuing Cannon says: "Very mild emotional disturbances are attended by abolition of peristalsis," and "like the peristaltic waves in the stomach, the peristalsis and the kneading movements (segmentation) in the small intestine and the reversed persistalsis in the large intestine all cease when the observed animal shows signs of emotional excitement."

From the foregoing it is logical to deduct the following: Given a good appetite and an appetizing dinner esthetically served, in pleasant surroundings, and with agreeable company, the digestion will be perfect, the stomach will be emptied quickly, the food will be passed rapidly through the small intestine into the colon, absorption of the nutrients will be rapid and complete, and the residue will be evacuated in normal time,—having a perfectly working gastro-intestinal canal and giving to the various tissues of the body the nourishment that is their just due. The converse of this would be equally true: Given, a poor appetite

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and unappetizing food served in a slovenly and disgusting manner, in disagreeable surroundings, in solitude or with unpleasant companions, perhaps with business worries pressing upon one or spiced with a domestic altercation, the saliva would be scanty, and an insufficient amount and poor quality of gastric juice and an inadequate supply of bile and pancreatic juice would be secreted. The stomach would be slow in emptying, and the food would pass tardily through the small intestine to the colon. The absorption would be slow because the food would not be well digested and because the retroperistalsis in the colon would not bring the nutrient material in contact with the absorbing colonic wall and, finally, the waste material would remain unduly long in the large bowel and an excessive amount of gas would form, overstretching and weakening the musculature of the bowel. It would also leave a somewhat lowered tone of the gastro-intestinal canal, and would fail to provide sufficient good nutrition to the various tissues of the body; and the victim of this sort of a meal would approach his next meal not quite so well prepared to dispose of it as he was before.

Let us imagine an endless series of such meals under similar circumstances, perhaps with bodily and mental fatigue added. I believe this would in time produce intestinal stasis in its most aggravated form. I believe conditions like the foregoing, with various modifications, are the cause of most of the cases of intestinal stasis that occur without mechanical causes.

There are many other factors that may have to do with the bringing on of stasis. Years of neglect to regularly evacuate the bowels is probably a potent factor in some cases. The weakening of the abdominal muscles by sedentary lives or tight-lacing is another

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etiological factor of importance, as is also a too exclusively meat diet with too little vegetables and fruits.

It is my opinion that the foregoing constitute, in the main, the causes of most of the cases of intestinal stasis,—in other words, that intestinal stasis is a preventible condition. It is probable that many of these cases have their origin in infancy and early childhood. There is a great responsibility resting on physicians who have the care of infants and children. It lies in their power to correct many of the dietary and hygienic faults of early life before the damage has been done.

A campaign of education is seriously needed along the lines of proper food, and the proper atmosphere which surrounds the taking of food. Many of the cases of intestinal stasis consult their physicians before the condition is very far gone. There is no doubt but most of these early cases could be cured by getting at the cause of the condition and training the patient and family to more enlightened methods.

That short-circuiting or colectomy has a large place in the treatment of such cases I am not prepared to believe. It is not a procedure that appeals to reason. The condition is entirely one of perverted function brought about by a long series of abuses. The condition in the severer cases affects the whole alimentary canal from stomach to anus. In order to restore, in part, the weakened intestinal walls, increased nutrition is sorely needed. The proposition to excise that portion of the colon which has especially to do with absorption of the nutrients seems theoretically to be about the worst thing that could be suggested.

It was not many years ago that surgeons were industriously performing gastro-enterostomies for atonically dilated stomachs when there was no obstruction at the pylorus. We have learned now how futile, and

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worse than useless, this operation is; and no one is doing it any more.

It is my opinion that very soon those surgeons who have permitted themselves to be stampeded into short-circuiting operations and colectomies for intestinal stasis will be wondering why they could ever have expected benefits to arise from such an illogical procedure.

The objections to this operation seem to outweigh any of the benefits that can be justly claimed. Considering the weakened condition of every patient in such an extreme condition as to require such a radical procedure and the extremely heroic operation which colectomy is always bound to be, one must look for a very high mortality. If the operation is survived, the dense mass of adhesions that can scarcely fail to follow would seem to the uninitiated as great an evil as the original condition.

Only a fractional part of the pathology is removed, and the segment excised is of such vast importance to the nutrition of the individual that nothing like robust health can ever be reasonably expected. Many cases seem to be benefited at first, but the improvement is short lived. One of two conditions by the very nature of the case is almost sure to occur after colectomy: (1) such a loose condition of the bowels that gradually failing strength will finally put an end to the scene; or (2) adhesions will produce a state bordering on obstruction, and the sufferer will continue to be as much a victim of stasis as before.

It is very questionable whether any case of uncomplicated intestinal stasis ought to be regarded as belonging in the realm of surgery at all. It would seem much more rational, considering the usual causes which produce the vicious chain of symptoms, to treat

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it on the broad ground of removal of the cause as far as possible.

A careful, continuous enlightened effort to put the victim of intestinal stasis on a more rational dietary and wholesome hygiene, and not to ignore the psychic factors that seem to play such an important rôle in its etiology will at least produce as much benefit as extensive operations.

It is fully realized that many of these cases reach such a status that to expect to restore the weakened intestinal musculature would be absurd. But even in this class of cases much might be accomplished in the way of the amelioration of the symptoms.

The real ray of light comes from prophylaxis and from the recognition of the early stages of stasis before irremediable changes have occurred. Any of these cases taken early and treated carefully and rationally might be restored to normal again. A weakened intestinal musculature can be made a normally strong intestinal musculature. A dilated and elongated intestinal canal may be restored to its original lumen and length. A stagnant bowel teeming with toxic material may be made to empty normally.

I admit that the treatment of cases of this type will demand courage, firmness, tact, and perseverance, but I leave it to any of my hearers whether you would not choose this method for yourselves rather than submit yourselves to colectomy.

The third class of cases of intestinal stasis—the cases where after long existence of the stasis there occurs a migration through the intestinal walls, giving rise to a subinfection and the production of adhesions, which cause obstructive symptoms,—naturally requires surgical procedures. Such procedures should be limited to simple relief of the obstruction.

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DISCUSSION

DR. JOHN E. SUMMERS (Omaha, Neb.): I heartily concur in practically everything Dr. Davis has said, but I think we can perform a successful minor operation in some of these cases, and that is the formation of an artificial fistula in the cecum with irrigation of the large bowel daily and sometimes twice daily. We may use in these cases Fisher's solution for irrigation, especially where there is evidence of acidosis. I believe this is a valuable addition to the treatment which Dr. Davis has advocated, and I have had some rather remarkable successes following the treatment. For many years, both in practice and in published papers, I have advocated this irrigation treatment. In one of my papers (Annals of Surgery, July, 1905) the subject was fully discussed.

DR. AMOS W. ABBOTT (Minneapolis, Minn.): I would like to report very briefly a case of intestinal stasis which is unique. The patient, a man, had for years what the Germans would call "inspirational spasm," a very unusual thing. I find only eleven cases on record. This patient would be taken with this sort of spasm. He would fill up his chest, and hold it in that condition so long that he would become cyanosed and fall over. He was badly constipated all of the time, and was worse just before the attacks. *X*-ray examination disclosed a hiatus in the colon, indicating stricture. He was relieved of the stricture, and has been driving a grocery wagon for over a year with no return of constipation.

DR. A. E. BENJAMIN (Minneapolis, Minn.): This is a subject which has not yet been settled; and if there is any one thing this Association should do, it should be to settle, if possible, unsettled surgical problems.

Last year I read a paper on this subject, and have been making some investigations since then. There is one thing we should determine, because these cases cause a great deal of trouble, and that is when to operate. The *x*-ray has given more evidence than any other one thing, and before we decide on operation we should try all methods, instead of doing as Dr. Summers advocates, namely, opening on the right side and draining and washing out the colon. We should put these patients in the knee-chest position and irrigate the bowel from two to four times a day, flushing out the colon and getting the colon and other abdominal organs in the proper position. At operation by separating adhesions or

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bands which interfere with the function of the colon and following out the treatment as outlined before operation, I find that we get satisfactory results in most of the cases.

Dr. Donald Macrae (Council Bluffs, Iowa): Of the cases that have come under my observation with this condition, which are few in number, most of them have gone through the treatment that Dr. Davis speaks of,—gymnastics and other things; but some are not benefited, and were these patients sitting here today and asked whether they would be willing to undergo a serious operation, such as colectomy, they would say "Yes." There are a few cases of this kind that are absolutely incapacitated for work and for everything else. They are melancholic, and all things considered, life is not worth living. While I realize this subject is still in its infancy, and has not been worked out thoroughly, it seems to me the results I have had from colectomy in four patients of this class warrant me in continuing the operation in selected cases.

Dr. Davis: How long is it since the operation was done?

Dr. Macrae: A year and a half ago. All these patients live near Omaha. They have gained in weight, and have improved so much that I feel justified in doing colectomy in future cases.

Dr. B. B. Davis (closing): The point I wish to make is, that colectomy is an illogical procedure because, in the first place, we take away the absorbing surface of the bowel. In the second place we are not justified in reaching conclusions on cases that have not been operated on two or three years. I believe most of the patients who have survived the operation are temporarily benefited. I do not think we can expect other than temporary benefit when we have to do with the static condition of the entire intestinal canal, and take away the most valuable portion of the colon; and I cannot believe we are justified in doing colectomy, although I have a number of patients who have been operated on nearly two years.

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SOME EXPERIMENTS WITH RUBBER GLOVES

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We have become so in the habit of the daily use of rubber gloves that for a number of years little has been written on the subject, except to emphasize their necessity. The facts which I wish to present have nothing to do with the consideration of the use of gloves as a protection to the patient or surgeon against infection. Most surgeons seem to have come to the conclusion that the use of rubber gloves is a necessity and that the protection which they afford against infection outweighs all other considerations. It is not my intention to combat the idea that this is a very proper conclusion on the part of surgeons; and undoubtedly it is almost universally accepted at the present time.

Notwithstanding this consideration, however, there are, no doubt, others than myself who have wished that their hands might be freed from the limitations of movement and touch which rubber gloves impose.

It is well known that a number of surgeons of international reputation for satisfactory technic have never used, or have discarded the use of, rubber gloves. Most surgeons who do not use rubber gloves themselves insist on their use by their assistants. Several conspicuous examples could be mentioned and probably occur to each of us.

What I have to present is entirely from the stand-

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point of the practical interference which gloves offer to the sense of touch. This interference is not limited to the purely tactile sense, but also interferes with our estimate of resistance and power of description. The interferences are partly due to the fact that, if the gloves fit properly and closely to the skin, in order to project the sense of touch a certain amount of muscular energy is used to overcome the elasticity of the gloves.

All sensations received by the gloved fingers must be classified as "projection of touch," and theoretically can hardly be expected to possess all the qualities which the mixed sense of touch has in addition to the purely tactile sense, the auxiliary use of the hairs, and the quality of discrimination.

While we are ready to admit that asepsis is of primary importance in operative surgery I always have a suspicion that we may be missing important information on account of some blunting of the sense of touch by the use of gloves. At least it is only fair that we know just what handicap the gloves give us, as well as to study the possibilities of discontinuing their use.

I wish to report a practical experiment in the use of rubber gloves. It seemed to me that the blind, who do all their reading with their fingers, would give an opportunity for observation on the effect of rubber gloves on the tactile sense which could not be gotten in any other way. With the co-operation of Superintendent H. C. Montgomery, of the Illinois State School for the Blind, and Principal Mrs. L. B. Inglis, of the High School department of that school, six of the best finger readers among the high school students were selected. There were three boys and three girls. The plan of all the observations was the same, namely, to select one hundred (100) words (thirteen lines) of

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unfamiliar text and new type which had not been used before, and to count the number of seconds required to read the one hundred words. The figures in the accompanying chart are seconds of time used in reading one hundred words. The text was Montgomery's *American History,* and was printed in the Braille, which consists entirely of raised dots pressed into heavy paper. The Braille type is based on six dots (1, 2, 3), that is, reading from the top to the bottom, then beginning again at the top (4, 5, 6). All of the letters are combinations of those six dots (1, 2, 3, 4, 5, 6).

Four series of observations were made.[1] The first series consisted of the observations before any experience in the use of rubber gloves had been acquired.

Observation 1.—The bare fingers were used. One hundred words of text were assigned to each pupil. The boys read in an average of 61 seconds and the girls in an average of 49 seconds, or in a general average of 55 seconds for the six pupils.

Observation 2.—The hands were covered with thin-weight rubber operating gloves. The three boys read one hundred words in an average of 77 seconds and the three girls in an average of 65 seconds, or in a general average for the six pupils of 71 seconds.

Observation 3.—The hands were covered with medium-weight rubber gloves, such as are most commonly used by surgeons. The three boys read one hundred words in an average of 107 seconds and the three girls in an average of 63 seconds, or in a general average of 85 seconds for the six pupils.

In this observation it will be noted that the three girls seem to have read more rapidly with medium-weight gloves than with thin ones. This apparent discrepancy is entirely due to the fact that pupil No. 4 had an easy, free-flowing passage, which was easily

[1] For details and summaries of observation see table which accompanies this paper.

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read. This explanation is emphasized by her results in Observations 2 and 4, where her record is perfectly consistent with that of the others.

There are a number of places where an individual reading would need correction in this way. Instead of being easier than the average it may have been more difficult, as, for example, a passage which contained an unusual number of names of unfamiliar persons or places. The average in seconds must be relied upon to give the correct estimate of speed.

Observation 4.—The hands were covered with thick-weight rubber gloves, such as are commonly used in dressing-rooms or by some surgeons in operating. Three boys read one hundred words in an average of 121 seconds, and the three girls in an average of 92 seconds, or in a general average of 106 seconds.

It seems reasonably evident from these observations that the sense of touch is blunted just in proportion to the thickness of the covering over the touch organs of the fingers, that is, the sense of touch with medium-weight rubber gloves covering the fingers, as compared with the bare fingers, is in porportion as 55 is to 85, or, to put it in another way, the medium-weight gloves seem to reduce the acuteness of the sense of touch by about 50 per cent.

The second series of observations was made on the same pupils after about two weeks' daily practice in the use of rubber gloves. I left them a supply of gloves with a request that they practice finger reading for a short time each day with the gloves on, and I am assured the request was carried out.

Observation 5.—The bare fingers were used. The boys read one hundred words in 64 seconds, and the girls in 38 seconds, or in a general average of 51 seconds.

By practice the general average was reduced four seconds, notwithstanding the fact that pupil No. 3 took almost twice as long to read as he did in Observation 1, and Observation 20

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shows his general average for bare fingers to be only 57 seconds.

Observation 6.—The hands were covered with well-fitting gloves of medium weight, and each glove was selected to fit the hand, which is the only difference between this and Observation 3. The three boys read the one hundred words in an average of 83 seconds and the three girls in an average of 48 seconds, or in a general average of 66 seconds. Comparing this general average with that obtained in Observation 3 it would seem that the two weeks' practice had improved the facility of reading through rubber gloves over 20 per cent.

Considering the great natural facility developed by the blind in using their fingers it would seem probable that this facility would not be further increased to any great extent by prolonged practice.

Observation 7.—The hands were covered with very loose, ill-fitting rubber gloves of medium weight and such as are frequently used by nurses and assistants and not infrequently on the hands of surgeons. The three boys read one hundred words in an average of 115 seconds, and the three girls in an average of 58 seconds, or in a general average of 86 seconds.

The lesson from this observation is plain. Gloves should be carefully selected to fit the hand, so that there will be no slipping or rolling of the rubber tissue on the fingers.

Observation 8.—The hands were covered with medium weight, well-fitting gloves containing an excess of talcum powder. The three boys read one hundred words in an average of 110 seconds and the three girls in an average of 46 seconds, or in a general average of 78 seconds.

This observation would seem to show that, while an excess of powder in the rubber gloves causes considerable diminution in the tactile sense, it is not as important a factor as loose or poorly fitting gloves.

In the third series of observations, instead of using paper books printed in Braille we used the brass sheets of Braille text from which the paper sheets are

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OBSERVATIONS IN FINGER READING WITH BARE FINGERS AND WITH RUBBER GLOVES BY HIGH SCHOOL PUPILS AT ILLINOIS STATE SCHOOL FOR THE BLIND, JACKSONVILLE, ILLINOIS

Observation Number	Each observation consisted of the numbers of seconds required for reading with the fingers, 13 lines, or approximately 100 words of Braille text, printed on both paper and brass. The text was new and unfamiliar and a new page was selected for each observation.	1 Carl Kunter	2 Frank Foster	3 Frank Thompson	*Boys' Average*	4 Marie Schaeffer	5 Johanna Crowley	6 Hannah Wessels	*Girls' Average*	AVERAGE
	Prior to any experience in the use of rubber gloves—									
1	Bare fingers	65	68	52	61+	45	52	50	49+	55
2	Thin gloves	72	100	61	77+	67	66	63	65+	71+
	Braille on paper—									
3	Medium gloves	92	130	100	107+	52	65	73	63+	85+
4	Thick gloves	90	180	95	124	76	123	77	92	106+
	After two weeks' daily practice with rubber gloves—									
	Braille on paper—									
5	Bare fingers	45	58	90	64+	35	35	45	38+	51+
6	Well-fitting gloves (medium), dry	55	80	115	83+	46	45	55	43	66
7	Loose-fitting gloves (medium), dry	70	105	170	115	50	55	70	58	86+
8	Excess of powder in well-fitting gloves	7[illegible]	95	160	110	45	45	50	46	78+
	Braille on brass sheets—									
9	Bare fingers	43	78	50	57	43	45	47	45	56+
10	Well-fitting gloves (medium), dry	55	70	80	68+	37	37	45	39+	54+
11	Well-fitting gloves (medium), hands wet	50	45	65	53+	30	35	32	32+	42+
12	Oil outside of gloves	65	102	100	89	43	44	41	42+	65+
13	Oil inside of gloves	80	90	100	90	47	56	52	51+	70+
14	Oil both inside and outside of gloves	52	74	70	65+	39	39	50	42+	54
	Braille on paper—									
15	Bare fingers	38	50	36	40+	20	38	32	30	35+
16	Oil outside of gloves	101	95	80	92	50	68	72	63+	77+
17	Oil inside of gloves	64	100	65	76+	42	55	52	50+	63+
18	Oil both inside and outside of gloves	65	90	58	71	55	58	57	56+	63+
19	Dry gloves, but paper text oiled	58	80	62	66+	45	45	57	49	57+
	General averages—									
20	Averages for bare fingers	47+	63+	57	56+	35+	42+	43+	40+	48
21	Averages for all medium gloves (dry)	67+	89+	114+	90+	45+	48+	58+	50+	70+
22	Averages for oil inside of gloves	65+	88+	73+	75+	45+	52+	52+	50+	68+
23	Averages for wet hands in gloves	69	105	68	80+	49	53	48	50+	65+
24	Averages for all glove observations	70+	99+	89+	86+	49+	56+	56+	54+	70+

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Fig. 1. Showing the three young ladies reading Braille text with fingers.

Fig. 2. Showing one of the young men reading Braille text with fingers.

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Fig. 3. School-room in Illinois State School for the Blind, where the observations were made.

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printed. These sheets, of course, are identical with the paper page, except that the raised letters are on the brass sheets instead of on the paper. The letters on the brass look much smoother and more definite than on the paper, but the teachers and pupils agree that the paper is more easily read.

Observation 9.—The bare fingers were used on Braille text on brass plates and one hundred words assigned to each pupil. The three boys read one hundred words in an average of 57 seconds, and the three girls in an average of 45 seconds, or in a general average of 56 seconds.

Observation 10.—The hands were covered with well-fitting medium-weight dry gloves, and one hundred words of Braille on brass plates were given each pupil. The three boys read their one hundred words of text in an average of 68 seconds, and the three girls in an average of 39 seconds, or in a general average of 54 seconds.

Observation 11.—In this observation the gloves were put on wet, that is, filled with water and put on the hands, and one hundred words of Braille on brass plates given to each pupil. The three boys read their one hundred words in an average of 53 seconds, and the three girls in an average of 32 seconds, or in a general average of 42 seconds.

In all other observations the gloves were used perfectly dry and no more powder used, except in Observation 8, than was necessary to get the gloves on.

It seems to be demonstrated in Observation 11 that gloves put on wet interfere less with the sense of touch than gloves put on dry. Each pupil remarked on this fact, saying that it was because the tissue did not slip about on the fingers so much.

Observation 12.—A perfectly refined mineral oil was put on the outside of the gloves, and brass sheets given to read. The boys read one hundred words in an average of 89 seconds, and the girls in an average of 42 seconds, or in a general average of 65 seconds.

Observation 13.—The same as Observation 12, except that the oil was put on the inside of the gloves. The boys read

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one hundred words in an average of 90 seconds, and the girls in 51 seconds, or in a general average of 70 seconds.

Observation 14.—The same as Observations 12 and 13 excepting the oil was both inside and outside the gloves. The boys read one hundred words in 65 seconds and the girls in 42 seconds, or a general average of 54 seconds.

Observation 15 was a control, and the bare fingers were used on paper Braille text. The boys read one hundred words in 41 seconds, the girls read it in 30 seconds, or a general average of 35 seconds.

The fourth series of observations was with oil in which the paper text, instead of the brass sheets, was used.

Observation 16 was the same as Observation 12, except that the paper text was used instead of brass. The boys read one hundred words in 92 seconds, and the girls in 63 seconds, or in a general average of 77 seconds.

Observation 17.—The same as Observation 13, except that paper text was used instead of brass plates. The boys read one hundred words in 76 seconds, and the girls in 50 seconds, or a general average of 63 seconds.

Observation 18.—The same as Observation 14, except that the paper text was used instead of brass. The boys read one hundred words of text in 71 seconds, and the girls in 56 seconds, or a general average of 63 seconds.

Observation 19.—The hands were covered with dry gloves, and the paper text was thoroughly oiled with mineral oil. The boys read one hundred words in 66 seconds, and the girls in 49 seconds, or a general average of 57 seconds.

Observation 20.—Consisted of the averages of all bare-hand readings (four observations and twenty-four readings). The boys had an average of 56 seconds, and the girls an average of 40 seconds, or a general average of 48 seconds.

Observation 21.—The average of six observations and thirty-six readings with the hands covered with medium-weight gloves put on dry. The boys had an average of 90 seconds, and the girls an average of 50 seconds, or a general average of 70 seconds, as compared with 48 seconds for the bare hands.

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That is, in an average of sixty individual observations it required an average of 22 seconds longer to read with medium-weight gloves than with the bare fingers, or, to put it another way, the time was increased 45 per cent by the use of gloves. This is the central point of the experiment. What effect on technic does such a considerable blunting of the sense of touch have? Are we as surgeons reading our text as well as we should?

Observation 22.—The average of four observations and twenty-four readings where oil was used in or on the gloves. The boys had an average of 75 seconds, and the girls an average of 50 seconds, or a general average of 68 seconds, or a slightly better average than with dry gloves.

Observation 23.—The hands were put into the gloves wet. This average contains, in addition to Observation No. 11, two additional observations or three in all, although two of them are not given in detail. There were eighteen readings of one hundred words each, and the boys had an average of 80 seconds, and the girls an average of 50 seconds, or a general average of 65 seconds.

This observation seems to show that medium-weight gloves put on wet interfere somewhat less with the sense of touch than any other plan, and that thoroughly dry gloves on thoroughly dry hands interfere with the sense of touch more than any other plan.

Observation 24 shows the average of all observations with gloves. The boys averaged 86 seconds, as against 56 seconds with bare hands, 90 seconds with dry gloves, 75 seconds with oiled gloves, and 80 seconds with wet gloves. The girls averaged 54 seconds, as against 40 seconds with bare fingers, 50 with dry gloves, 50 with oiled gloves, and 50 with wet gloves. The general average was 70 seconds, as against 48 seconds for bare fingers, 70 seconds for dry gloves, 68 seconds for oiled gloves, and 65 seconds for wet gloves.

CONCLUSIONS

1. The use of medium-weight rubber gloves requires the blind to use an average of 22 seconds more

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in reading one hundred words of Braille than with the bare fingers,—namely, 48 seconds, with the bare fingers, and 70 seconds with medium-weight gloves; or, in other words, there is a loss of nearly 50 per cent in the sense of touch, judging from the results of this experiment.

2. The tactile sense is materially improved by putting on wet instead of dry gloves, the difference being an average of five seconds, or a little less than 10 per cent. Gloves put on with oil on the hands give a slight improvement over dry gloves,—namely, 68 seconds as against 70 seconds.

3. The tactile sense diminishes in direct proportion to the thickness of the gloves, as shown in our first series of observations, where thin gloves showed an average of 71 seconds, thick gloves showed an average of 106 seconds, as against an average of 48 seconds with the bare fingers.

4. A marked improvement in the tactile sense is brought about by the use of carefully fitted gloves, as shown in our second series, where, by care in fitting, the average was reduced from 70 seconds to 66 seconds.

5. As a final conclusion we may say that the final result of the experiment is, that gloves put on wet give the most favorable opportunity for exercising the sense of touch, and gloves put on dry give the least favorable.

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